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MEDICAL RESEARCH COUNCIL.

INDUSTRIAL

FATIGUE RESEARCH BOARD.

REPORT No. 29.

The Effects of Posture and Rest in Muscular Work.

A.—Comparison of the Energy Expenditure of a woman carrying loads in eight different positions, by E. M. Bedale, M.A.

B.—The Influence of Rest-pauses and Changes of Posture on the capacity for Muscular Work, by H. M. Vernon, M.D.

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PREFACE.

The increasing substitution of mechanical for manual processes—probably the most prominent feature of modern industrial practice—has been accompanied by a gradual modification in the nature and extent of the demands made on the worker. Machinery is essentially labour saving, and the introduction of every new mechanical process reduces the number of workers in industry, the performance of whose tasks is dependent on heavy bodily exertion. Even when machine processes require the expenditure of considerable muscular work, as is often the case, these almost always entail a largely reduced effort in comparison with the original manual processes.

The general tendency therefore of modern industrial practice is the gradual disappearance of heavy manual operations, and the retention only of manual operations, usually involving light and dexterous work, which have an important bearing on the quality of the article manufactured. Simultaneously, if the whole of industry is considered, the mental effects of the work on the operative tend to become relatively more important than the bodily effects, with the consequence that many practical problems are most appropriately studied by psychological methods.

In spite, however, of the great displacement of manual work by machine work, there still remain many occupations in which the demands made on the worker are mainly physiological, and these, though naturally commonest in the "heavy" industries, are by no means confined to them. In particular, the lifting and carrying of loads are necessary operations in varying degrees in almost all factories.

The following Report contains the results of two investigations made into work of this type. In the first of these, eight common methods of carrying a load are compared. It is shown that these have a definite order of merit, as demanding different expenditures in the chemical exchanges of the body, and that these physiological "costs" are related to physical

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causes, particularly to the different degrees to which the body's normal centre of gravity is displaced. It is important to note that this Report deals only with the methods of carrying, and hardly touches at all upon the maximum weight of the load that can be carried without discomfort, a question which the Board hope to explore in the near future.

The second investigation deals with the effects of maximal effort in lifting and pulling. The principal points that emerge are, first the existence of an optimum height from the ground at which the greatest effort can be exerted, and of a "weak spot" nearer the ground, secondly the beneficial influence of changes in posture on the maintenance of pulling capacity, and lastly the beneficial influence of rest pauses interpolated between the pulls.

December, 1924.

PART A.

COMPARISON OF THE ENERGY EXPENDITURE OF A WOMAN, CARRYING LOADS IN EIGHT DIFFERENT POSITIONS.

By E. M. BEDALE, M.A.

(with an introductory note by E. P. CATHCART, M.D., F.R.S.).

CONTENTS.

INTRODUCTORY NOTE.

Problems of load carrying are as old as man, but their scientific investigation is only a matter of recent times.

In spite of the fact that in many modern factories mechanical conveyers have been introduced, and that their use is extending, it will be a very long time before hand labour is wholly superseded in the transport of materials about workrooms. Several questions emerge in the consideration of the problems of such transportation. What is the best method of carrying the load ? What is the maximum economic load, i.e. the greatest load that can be carried without unduly taxing the subject ? What is the maximum period of work ? What value have rest pauses ? These and other questions arise. All are of importance. The difficulty lies in obtaining definite answers to these questions by mere observation, even though amplified by the subjective judgment of the operative. In fact it may be stated that, owing to the number of psychic factors involved, it would be well nigh impossible to get a clear-cut answer in this way.

Fortunately, within recent times a method has been elaborated which has enabled scientific workers to assess, with a considerable degree of accuracy, the amount of energy expended by a subject in the performance of a given piece of work, or for the working part of his whole daily routine.

The output of energy by a subject may be determined in two ways; either by direct or indirect calorimetry. The common unit of measurement utilised for all assessments, that is, for the expenditure both at rest and during work, is the large or kilo Calorie.

In direct calorimetry, the subject is enclosed in a special chamber which is so arranged that all the *heat* given off by the subject can be measured directly as heat. As a rule, combined with the special apparatus for the determination of heat, all the air passing into and coming from the chamber can be measured and analysed, that is, direct and indirect calorimetry are combined. The objection to such calorimeter methods for the examination of a subject performing ordinary work, is that the chamber is apt to limit his movements and the conditions are always artificial.

In indirect calorimetry, the energy output is computed from *chemical* measurements of the respiratory exchange. By the Douglas–Haldane method, when a determination is being carried out, the subject wears a mouth-piece or a face-mask equipped with two one-way light rubber valves. One of these valves communicates directly with the outside air and serves for inspiration; the other valve, which is connected by means of a length of rubber tubing to a large gas-tight bag carried on the back of the subject, serves for expiration. The expired air of the period, usually two to three minutes during a work experiment, is then carefully measured and a sample is drawn off into a gas sampling tube for analysis. The gas is analysed in a Haldane apparatus and the amount of oxygen (O_2) used, and the amount of carbon dioxide (CO_2) given off in the period of experiment, are determined. The ratio of CO_2 excreted to O_2 absorbed is then obtained. The volume of oxygen utilised in a given period, say one minute, is converted into an assessment in heat units by multiplication with a factor determined experimentally and based on the CO_2–O_2 ratio.

Indirect calorimetry by this method gives much greater license to the subject and multiplies the varieties of work which can be dealt with. It has been shown by experiment to be quite as accurate as the direct method. The objections raised against it are, that the period of collection of a sample is too short, and that the work done during the time of sampling may not be a fair sample of the work done throughout the working period. As regards the first objection, it has been shown conclusively that accurate determinations can be made from a sample collected over less than one minute. The second objection is a much more serious one and is frequently only too well founded, more especially where, for example, the total energy output for a day is calculated from a few samples. Where the work is varied in nature, the results are of doubtful value unless, as the result of prolonged personal investigation, the operations of the subjects are carefully determined, and the average duration of

each type of operation and the average rest pauses are also noted and included in calculation. But if the subject under investigation is a trained laboratory worker, used to the apparatus and the technique employed, and if in addition the type of work studied is of a repetitive nature, such as walking, marching, working a pedal or lever ergometer (work-measuring apparatus), and is carried on continuously during the period of observation, the results can be depended upon to give a true index of the energy expended on the performance of the particular operation. The high degree of accuracy attainable is shewn by repeating individual experiments. Thus the following table (Table I) gives records of 12 out of the 15 repeat experiments, carried out in the present investigation. In these, all conditions were reproduced as closely as possible, but the duplicate experiments were done at intervals, sometimes, of many weeks.

TABLE I.—*Duplicate Experiments.* *O_2 consumption per min., net.*

cc.		cc.		% Difference.
603	..	604	..	+0·2
594	..	602	..	+1·3
557	..	547	..	−0·5
615	..	623	..	+1·3
499	..	496	..	−0·6
534	..	534	..	0·0
602	..	608	..	+1·0
616	..	608	..	−1·2
623	..	620	..	−0·5
607	..	604	..	−0·5
530	..	537	..	+1·3
480	..	493	..	+2·7

Another objection, which has been raised repeatedly by those who know little or nothing of the technique of the method, is that all results obtained are commercially useless because they are not carried out on skilled operatives. This is not the case in the majority of instances. True, if one desired to obtain some idea of the average energy cost of a particular industrial operation, it would naturally be foolish to take a subject who knew nothing about the particular series of operations and then report as if the results were a true index of the work done in the particular operation. There are, however, many forms of simple work which can be carried out in a variety of ways, where if the *relative* cost of the various methods of the performance of the work is to be accurately determined, it is a positive benefit to employ a subject who is skilled in none of them. Then, and then only, is it possible to obtain *relative* values of real worth. Such a simple type of work is the one dealt with here, where the sole object of the investigation was to determine, *not* the maximum load the subject could carry, but the relative cost in energy of the various methods of the carriage of loads of varying weight.

The fact that no attempt has here been made either to determine the maximum load or the mechanical efficiency of the performance of the work must be emphasised. So far no specific

regulations exist in Britain which definitely limit the loads to be carried by women, except in the pottery industry, where there are some restrictions in the case of women and young persons. France has definitely limited the load to be carried by women of over 18 years of age to 55 lb. It must not be overlooked, however, that certain workers in this country have made recommendations. Thus Dr. Janet Campbell, in the *Report of the War Cabinet Committee on Women in Industry*, 1919, p. 222, writes: "It is obviously difficult to make definite suggestions in regard to the weights which can safely be handled by women, but given reasonable conditions and good physique women and girls over 18 years of age have been found able to handle weights up to 50 lb. . . . without difficulty."

As regards the mechanical efficiency of performance, i.e. using the methods of the engineer to determine the overall thermal efficiency, it is questionable if the values obtained are of any interest beyond a purely academic one. It is quite true that a machine runs most efficiently when fully loaded and there is a certain amount of evidence to show that within certain limits the human being does in this respect resemble a machine. But it cannot be too strongly emphasised that the human being is no mere machine, and that the mechanical efficiency of performance is not of primary importance. The performance of work by the human being is not a mere function of the combustion of so much fuel in the muscles involved in carrying out the particular movements, and therefore its expression as the relation of external work done to oxygen utilised, when both these values are expressed in terms of heat units, is beside the point. Indeed, the application of the word efficiency with its definitely limited mechanical meaning to the results of human activity is almost certainly not valid. A term like effectivity, which may be held to connote all the factors which go towards effective and useful performance, would be a better expression for the ideas that are at present included in the general term human efficiency.

E. P. C.

Previous Work.

A good deal of work has been done already by indirect calorimetry on the metabolic cost of carriage of loads, but, so far as I am aware, no previous study has been made either of the carriage of loads by women or of the relative cost of carrying loads by different methods or in different positions.

Zuntz and Schumburg (1901) carried out a long series of observations on the cost of soldiers marching loaded, and they came to the conclusion that the energy cost increased almost proportionately to the masses moved. They noted also that the *position* of the load carried was of prime importance, although

they did not deal specially with this point. *Brezina and Kolmer* (1912) found that the cost in energy was not adversely influenced by loads up to 21 kilograms, i.e. this amount of extra dead weight could be carried as economically as so much live weight. Heavier loads they found brought about both an absolute and relative increase in the energy output. *Brezina and Reichel* (1914) came to the conclusion that after the load carried exceeded 19 kilograms in weight the increase in energy output was proportional to the square of the load difference. *Cathcart and Orr* (1919), in the course of their investigations into the energy expenditure of the infantry recruit in training, studied the influence of changes in the position of the load carried by the soldier. Their results showed that if the load were not evenly distributed or if the position of the pack was altered from the normal position, there was an increased energy expenditure. *Cathcart, Richardson and Campbell* (1923) investigated the question of the maximum economic load of the soldier and found that under laboratory conditions (i.e. where there is no question of interfering variations in ground or weather) the economic load is equivalent to about 40 per cent. of the body weight. *Dr. Eileen Hewitt* and myself (1923) carried out a preliminary investigation of the relative cost of four different methods of carrying by women. We found that, of the methods tested, the carriage of the weight on the shoulder gave the best results when the loads were fairly heavy—40 or 50 lb. or more. The present paper includes some of the material so gathered, and the investigation is extended along the same lines.

Present Work.

The experiments here reported were designed to be a study of the comparative physiological costs of carrying different loads by various methods. For experimental accuracy, and as there was no intention of determining the maximum load which a woman could carry, but merely the relative cost of the different methods of carriage, the observations were made on one trained laboratory subject. The conditions, which were as closely controlled as possible, were as follows :—

The place in which the experiments were carried out was a broad well-lit corridor, granolithic floored, with wide turning room at each end, giving a circuit of exactly 100 yards. It was well ventilated ; the cooling power, by dry kata-thermometer, was from 7 to 9. The subject wore heavy shoes with rubber studs to eliminate the foot-soreness which otherwise resulted from walking with weights on a springless flooring.

The time was between 10 a.m. and noon, for all experiments, except one series which were carried out during the afternoon. All were done about two or three hours after a light meal.

Eight modes of carriage were tested ; these will be described successively and their metabolic costs will be given.

The loads carried were 20, 30, 40, 50, and, in two cases, 60 lb. All of these are light loads compared with those often carried by women in factories. This course was deliberately adopted, since, in a preliminary study of differential costs, the absolute weights carried are of secondary importance, while for accurate experimentation it is essential that the work should be easily within the subject's compass, so that the pace may be kept up, and kept up without causing respiratory derangement by excessive effort.

The pace, about 2·8 miles per hour, was chosen on the results of preliminary trials. It was judged to be the maximum that a woman would attempt to keep up continuously. The circuit of 100 yards was performed in 73 seconds, a metronome being used to keep the rhythm. On the whole, the speed and continuity of the work were probably greater than is common in industry.

The experimental procedure was the performance of one hour's work, at the set pace, with a given load and mode of carriage. In order to represent, approximately, factory conditions of labour, the load was not carried continuously, but each circuit of 100 yards with the loaded tray or board, etc., alternated with another complete circuit, in which the subject carried only an "empty." The walking of each circuit, as already noted, occupied 73 seconds. The depositing of the load and picking up of the empty, and vice versa, were so arranged as to occasion practically no raising and lowering of the weight, and occupied about 4 seconds and 6 seconds respectively. *Three samples* of expired air, for measurement and analysis, were collected during the hour, after 20, 40, and 57 minutes' work. The duration of each sample was 2½ minutes, and consisted of the air expired during one loaded circuit, the unloading, and the unloaded circuit. *The work performed* during this sample was calculated in horizontal kilogrammetres from the total weights (body plus load or empty) transported over a distance of 200 yards, disregarding the very slight amount of "positive" and "negative" work involved in laying down the load and taking up the empty.

The values given for each load in Tables V—XIII are the averages of the three samples so taken in each experiment. The dispersion of these three values is, and should be, relatively small, since the first sample was taken after fully 20 minutes, so that the subject had thoroughly settled down to work. · In the course of the investigation, 15 experiments were duplicated, in some cases because the results were surprising, and technical error was suspected. Table I shows that 12 of these 15 confirmed the original determinations with remarkable accuracy, and in these cases the original value was accepted ; in the other three, error was demonstrated and the second determination was the one accepted. It would seem that, apart from technical

flaws, the subject's energy expenditure in a given task, under the same conditions, was very constant, and by using the first (or second) determination, any effects of special training (*see* below) were eliminated. For the same reason, the increasing loads were not taken successively, but in accidental order, and experiments on the several modes of carriage overlapped each other, and were spread over a period of months. However, in considering Table I, it has to be remembered that each figure is the average of three sample determinations, so that the correspondence of the pairs of "smoothed" values gives a rather exaggerated picture of physiological stability and technical accuracy. From the standard deviations of the oxygen determinations quoted in Tables I, II, and III, the actual dispersion of the individual observations can be gauged. It was highest in walking unloaded, being 22·7, (corresponding with a coefficient of variation of 5·3 per cent.). In the work experiments, the average dispersion of the three samples was 17·3 on the 0_2 values. To avoid overloading the tables, the averages of the three working samples are given without details of the individual samples or their dispersions.

The subject (Ht. 176 c.m. Av. wt. clothed 56 kg. Surface area 1·66 sq.m. Aetat 29) was in good health, and though under weight was probably not inferior in physique and fitness to the average woman in industry. She was a competent long-distance walker, but otherwise was unathletic, with poor muscular development of the arms and shoulders. She was unaccustomed to any method of load-carrying, except, when on holiday, that of a knapsack under 20 lb. This was an advantage, since the aim, as already stated, was to compare the differential costs of a variety of modes of carriage. No woman could be trained equally to all modes, and if trained to one only, she might be expected to have acquired habits tending to minimise the cost of that mode, while perhaps greatly exaggerating the cost of another, especially dissimilar. Comparable results could not therefore be obtained from any one such woman, while it would be inadmissible to compare data drawn from experts in different types of labour, unless a sufficient number of subjects could be obtained, in each group, to obviate personal factors. For a preliminary analysis, what was required was not special training, but merely a fairly well educated "motorial sense,"* enabling the subject to accommodate the load and the body to each other, so that in each case the work should be performed with the least exertion possible, under the existing conditions of muscular development and so forth. Good co-ordination would seem to require the elimination of extraneous movements, together with such a disposition of the load as to

* Motorial sense (Halliburton) = kinaesthetic sensations . . . "those by means of which we appreciate the position and movement of parts of the body, especially the limbs. . . . They are excited by the stimulation of sense organs in the muscles, in their tendons and in the tissue lining the smooth surfaces of the joints." *McDougall, Physiological Psychology.*

ensure the balance of the loaded body (in standing or walking) with the minimal muscular contraction, and with the minimal restriction to the movements of the limbs and the chest-wall. A trained worker should acquire a high degree of such co-ordination in her own line, but her metabolic costs would probably be affected not only by the more perfect "knack," but also by the development through use of the special muscles required, and by other factors.

Calculation of Net Working Metabolism.

It is usual, in presenting data for the metabolic costs of muscular work, to give the net costs above a selected base level, which is measured on each occasion as a preliminary to the work experiment. This is necessary, because, in spite of all precautions, uncontrollable factors influence the general level of metabolism, causing it to vary somewhat from day to day. The deduction of the day's standard from the gross working costs makes for the elimination of such chance variations in the final net values, and makes them, therefore, more comparable with determinations made upon other days.

Several base lines may be taken. In such a piece of work as the present, there are three alternatives :—

1. The true basal metabolism, measured with the subject at complete rest, at least twelve hours after food. This is the most constant level, and is often the best base-line over which to measure rises of metabolism. In all cases, it is desirable to know a subject's average basal metabolism, and the limits within which it commonly varies, for comparison with other subjects.

2. The metabolism measured with the subject lying still, after half-an-hour of rest, just before beginning the work experiment. This gives a higher level than the post-absorptive basal, and is more variable, being influenced by previous activity, diet, digestive states, etc. As these are just the fluctuations most likely to blur more significant variations, the deduction of this base level is especially suitable when it is desired to compare the rather fine differences of cost which arise when the same external work is performed by various methods.

3. The metabolic costs of the performance of a series of muscular movements, similar to those which will accomplish the specific external work, may be used as base line; e.g. the cost of walking at the experimental pace, with no impedimênta except the respiratory apparatus, may be deducted from the gross costs of load-carrying.

This last method was at first adopted in these experiments, but was soon changed for the second. The net values given in the tables are found by deducting the pre-work resting values. The averages of all three base-line determinations are, however,

given in this section. The average cost of unloaded walking has been used in the rough computations of industrial output, etc., at the foot of Tables V to XII.

Many determinations of the true (post absorptive) resting metabolism have been made on this subject. The average on consecutive days for seven weeks, during which load-carrying experiments were being carried out, was as follows :—

TABLE II.—*Basal Metabolism. Post absorptive.*

CO_2 per min.	O_2 per min.	Standard Deviation.*	Cal. per 24 hours.	Cal. per hour per sq. m.
141 cc. ..	174 cc. ..	7·4 ..	1225 ..	30·75

The majority of the experiments were preceded by a determination of the resting metabolism, in this case not the true basal, but 2½ hours or more after a light meal. The average of these determinations, for the first four sets of experiments, was as follows :—

TABLE III.—*Resting Metabolism before Work. Average.*

CO_2 per min.	O_2 per min.	Standard Deviation.*	Cal. per hour. per sq. metre.
186 cc. ..	219 cc. ..	12·4 ..	38·2

Nine of the earlier experiments were not preceded by a determination of the pre-work resting metabolism; in these cases (marked with an asterisk in the tables), the average resting value given in Table III is deducted from the working expenditure, while in all others, the value proper to the individual experiment is deducted. The difference is not great.

A number of experiments were preceded by a determination of the cost of walking at the experimental rate with no external load beyond the Douglas bag and mask (weighing 6½ lb.). The average readings, with pre-work resting value deducted, were as follows :—

TABLE IV.—*Walking. Average Net Values.*

CO_2 per min. cc.	O_2 per min. cc.	Standard Deviation.*	Cal. per. min.	Hor. kgm. per min.	O_2 per hor. kgm. cc.	Gr. cals. per hor. kgm.
343 ..	421 ..	22·7 ..	2·01 ..	4403 ..	·096 ..	·46

THE EXPERIMENTS.

Mode 1. Tray carried in front of the body. (*See plate facing page* 27.)

Mode 2. Tray carried in front, but with the weight taken off the arms by a strap passed round the shoulders to the front corners of the tray.

Mode 3. Weight tied up in equal bundles, and carried at the sides in either hand.

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* On oxygen determination.

Mode 4. Weight distributed over a board carried on the left shoulder.

Mode 5. Weight carried in a tray on the left hip.

Mode 6. Weight carried in a rucksack.

Mode 7. Weight divided between two pails and carried on a shoulder-yoke.

Mode 8. Weight carried in a tray upon the head.

Mode 1.

The practical convenience of tray-carrying makes it a method very common in domestic and factory work. For short distances and irregular work it is satisfactory. For long distances or long-continued regular transportation of heavy material, it has certain physiological disadvantages. The most obvious of these is the local fatigue of the wrists and arms, which must for every worker set a limit somewhere for the load or the distance. There may also be unpleasant pressure on the abdomen, pubis or thighs. But equally important factors affecting the cost and the sense of fatigue, in this and all modes of carriage, are the general alterations in the bodily posture and the gait. A rather detailed analysis of these factors in this first instance, may perhaps make clear the points at issue in every carrying-method.

In carrying a heavy tray, the tendency is to compensate for the load added in front of the body by throwing back the trunk so that the normal balance on the arch of the foot is easily maintained. The knees are slightly flexed, and from the knees to the head the long axis of the body falls back in an oblique line. The standing posture naturally and always assumed illustrates this. In walking, the knees are kept slightly slack: to extend them at every step requires exertion and results in painful jarring to the spine. With heavier loads, the shoulders may be dragged forward, and then the flexion of the knees is increased, to bring back the centre of gravity over the base of the feet. A person carrying a very heavy load in front is obviously chasing his own centre of gravity as he moves, and the muscular counteraction to the tendency to fall forward must be a considerable element in the cost of such a method of transportation. Closely connected with this is the question of the interference with the natural gait. In these experiments, in walking with 50 lb. load, the tray had to be carried pressing against the upper part of the thigh; that is, the thighs were supporting a part of the weight, and the free movement of the legs was therefore impeded. If the co-ordinations of the normal gait may be assumed to be mechanically and physiologically efficient, such interference should mean that all the muscles used in locomotion* are working

* "With every step, some three hundred engines have to be started, regulated, and stopped."—Sir Arthur Keith, *Engines of the Human Body*.

at some disadvantage. As a result of prolonged toil, the erect carriage and free gait may be lost; in such cases the habitual slouch is probably more metabolically economical than a deliberate resumption of the upright position; in other words, the whole locomotor system has adapted itself to the conditions under which it has most frequently to work.

With heavy loads, there is the further question of some degree of interference with the respiratory and circulatory functions through "fixation of the chest," when the trunk is kept as rigid as possible to give the muscles a greater mechanical advantage. It is common experience that a man transporting, for a short distance, a load only just within his compass, holds his breath in doing so, and has an increased and often laboured respiratory rate afterwards. The metabolic costliness of work in which the static element predominates has been shown (*vide* Cathcart, Bedale & McCallum) and the strain imposed on the organism by efforts requiring a general static contraction of thoracic and abdominal muscles is well known to gymnasts and athletes.

Theoretically, there would thus appear to be three general considerations to be borne in mind in studying all modes of carriage—the displacement of the centre of gravity, the interference with the gait, and the degree of thoracic fixation.

In each case, an estimate must be formed of the operation of these factors, and finally an examination must be made to see whether any correlation can be established between them and the metabolic costs and other measurable physiological effects.

Returning to Mode 1: the chief subjective symptom of fatigue was a slight ache in the lumbar region, but with the heavier loads there was also some pain in wrists and elbows. The tray measured 21 in. by 16 in., and weighed 4·8 lb. It was gripped by the hands in the middle of its sides, and was carried most easily in the horizontal position. The arms were fully extended, the joints taking the strain; this being obviously preferable to continuous static contraction of the muscles of flexed arm. The full output of work was not maintained, owing to a reduction of the stride with 40 lb. and especially with 50 lb. Since a stopwatch could not be carried, the subject did not know that she was losing pace. This reduced output is the usual manifestation of fatigue. From the point of view of experimental lucidity, it would perhaps have been preferable to eliminate this factor, keeping up the output by speeding up the metronome to suit the shortened stride. In later experiments this was done, but nevertheless the loss of pace with the heavy loads occurred in several modes of carriage, and must be taken into account in judging the efficiency of a method. Recalculation of the figures of the earlier experiments in various ways has shown that the variations of the work output, which are most marked in the first three modes, do not materially affect the position of their "cost curves" relative to those of the other modes of carriage.

TABLE V.—*Mode* 1. *Tray carrying I. Average net values.*

Load.	CO_2 per min.	O_2 per min.	Cal. per min.	Hor. kgm. per min.	O_2 per hor. kgm.	Gr. cals. per hor. kgm.
lbs.						
20	394	464	2·26	4643	·100	·49
30	475	522	2·58	4794	·109	·54
40	527	613	2·99	4905	·125	·61
50	587	675	3·30	4738	·142	·70

Approximate estimate of industrial output and physiological cost by Mode 1.

Load.	Material transported 100 yards in 1 hour.	Physiological cost in excess of cost of walking at same pace.
lbs.	lbs.	Calories per hour.
40	920	58·7
50	1070	77·3

Mode 2.

In this series, a broad webbing strap was passed round the subject's shoulders and fixed at the front corners of the tray. The hands were used only for steadying the load. The strap was adjusted to the level found by trial to be most comfortable, so that the tray pressed against the abdomen at umbilical level, leaving the thigh movement clear. The weight pulling through the strap tended to bring the shoulders forward ; to compensate this, and to meet the pressure of the weight inwards, the lumbar curve was increased, while the knees were flexed. It was much easier to carry the tray at a slight tilt down and in towards the centre of the body ; in the horizontal position, the tug on the shoulders was greater, and the maintenance of balance more difficult.

The subject preferred this method to the previous one. There was no local discomfort, but breathing seemed to be a little forced by the partial fixation of diaphragm as well as shoulders.

TABLE VI.—*Mode* 2. *Tray carrying II. Average net values.*

Load.	CO_2 per min.	O_2 per min.	Cal. per min.	Hor. kgm. per min.	O_2 per hor. kgm.	Gr. cals. per hor. kgm.
lbs.						
20	397	473*	2·29	4588	·103	·50
30	433	522	2·53	4713	·111	·54
40	519	604*	2·95	4935	·122	·60
50	564	656*	3·20	4992	·131	·64

* *Average* pre-work deducted.

Approximate estimate of industrial output and physiological cost by Mode 2.

Load. lbs.	Material transported 100 yards in 1 hour. lbs.	Physiological cost in excess of cost of walking at same pace. Calories per hour.
40	920	56·1
50	1150	71·3

Mode 3.

In this set of experiments the successive loads were divided equally, tied up into bundles giving a convenient grip, and carried at the sides, one in either hand.

By this method, there is no disturbance of the balance of the body, and the movement of the legs is not interfered with. Inasmuch as the erect carriage and free gait are preserved, such carrying should be physiologically economical. But for long-continued regular work it has, like Mode 1, the disadvantage of involving marked local fatigue of the arms, together with probable bruising of the legs by the loads. Further, with heavy weights, the drag on the shoulder-girdle is sufficient to require some fixation of the muscles of the chest and throat; the carrying of a maximum load for a short period was found to produce a laboured type of expiration.

The subject felt the local strain severely, particularly on account of an elbow injury received in childhood which altered the carrying angle. This strain is probably the reason for an unconscious slacking-off of pace, which helps to produce the almost flat performance of work in hor. kgm. for loads of 30, 40, and 50 lbs.; this effect, however, being much exaggerated by the fact that the subject was herself losing weight. [Contrast the figures for hor. kgm. in the first part of Table VII. with the estimated external work per hour, below it]. The validity of the experiments on 40 and 50 lb. loads is, therefore, very questionable. Since there was no "empty" to be transported in the unloaded circuits, the work performed should have been, in any case, rather less than that performed by other carrying methods.

TABLE VII.—*Mode* 3. *Carrying two bundles of equal weight. Average net values.*

Load. lbs.	CO_2 per min.	O_2 per min.	Cal. per min.	Hor. kgm. per min.	O_2 per hor. kgm.	Gr. cals. per hor. kgm.
20	373	455	2·20	4589	·099	·48
30	369	492*	2·33	4883	·101	·48
40	433	534*	2·57	4873	·110	·53
50	560	667*	3·24	4858	·137	·67

* *Average* pre-work deducted.

Approximate estimate of industrial output and physiological cost by Mode 3.

Load.	Material transported 100 yards in 1 hour.	Physiological cost in excess of cost of walking at same pace.
lbs.	lbs.	Calories per hour.
30	690	19·3
40	850	33·6
50	1060	73·5

Mode 4.

In this series, the weight was distributed upon a board measuring about 36 in. by 10 in., and was carried on the left shoulder, the left hand resting far back on the hips, and the right hand lightly steadying the board in front. A load so carried entailed displacement of the centre of gravity to the left, and to maintain the balance on the feet with the minimum of muscular contraction, the head and shoulders were thrown proportionately to the right. In standing, the feet were slightly parted, and in walking, they were also placed rather further apart than normally; the lateral load seemed to demand a broadening of the base of support. In addition to the lateral curvature of the spine, the head and shoulders were thrust forward, the load balancing with rather more than half its length behind. The reason for this pose—which was at once adopted because it alone seemed comfortable—was probably that the balancing of a loaded board on the ridge of the shoulder was precarious and painful; while if the shoulder was thrust forward and downward, the load rested securely on the pad of muscle above the shoulder-blade, and bruising was avoided.

The ease with which the load was adjusted so that the total weight fell squarely about the mid-line between the feet, meant that the lower limbs had freedom; the normal stride could be taken with all loads. The fixation of the chest was negligible. The subject liked this method, and was able to carry 60 lb. at the fixed pace with less difficulty than 50 lb. had occasioned in any of the first three modes. There was no local fatigue.

TABLE VIII.—*Mode 4. Carrying on the shoulder. Average net values.*

Load. lbs.	CO_2 per min.	O_2 per min.	Cal. per min.	Hor. kgm. per min.	O_2 per hor. kgm.	Gr. cals. per hor. kgm.
20	364	428	2·08	4758	·090	·44
30	443	547*	2·63	4844	·113	·54
40	481	609*	2·92	5060	·120	·58
50	480	608*	2·91	4987	·122	·58
60	599	778*	3·70	5390	·144	·69

* *Average* pre-work deducted.

Approximate estimate of industrial output and physiological cost by Mode 4.

Load.	Material transported 100 yards in 1 hour.	Physiological cost in excess of cost of walking at same pace.
lbs.	lbs.	Calories per hour.
40	900	54·4
50	1100	54·1
60	1380	101·8

Note: pace was lost with the 50 lb. load and the subject's body-weight was about 2 kgs. below normal at this time, with the result of bringing down the hor. kgm. per min. to about the same as that of the 40 lb. load; but the cost per kgm. is proportionately low; contrast with Tables V and VII in this respect. The slight shortening of the stride was not due to fatigue and could have been corrected had the subject been aware of it.

Mode 5.

The next method examined was, like the last, one commonly used by women in industry, viz., carrying on the hip. Like the last also, it demands a large deviation from the erect carriage of the body to compensate for the lateral load. The breadth of the burden and the angle at which it has to be carried affect the extent to which such compensation is required. If narrow, and capable of being " tucked under the arm," a comparatively slight lateral bending of the head and shoulders will suffice to bring the centre of gravity over the base of the feet. In these experiments, however, a more extreme case was taken; a tray 16 inches wide was carried in, approximately, the horizontal position. To balance this, the body was bent from the hip, and the right arm was held out from the body. In the posture thus naturally assumed, a true balance on the slightly parted feet was maintained with the minimum of muscular contraction and no subjective sense of strain. But since the load was resting on the wing of the pelvis, the left leg could not move freely, and actually in walking it functioned as a peg only, the feet being planted apart and at dissimilar angles, in a characteristic lame fashion. Thus, in respect of both body-displacement and interference with gait, this mode would seem to have physiological disadvantages. Natural breathing was also interfered with to some extent, since the left side of the thorax was stretched, while on the right side the floating ribs were pressed against the pelvis.

The subject found this hip-carrying distinctly tiring. There was some fatigue of the left arm, bruising of the hip, and footsoreness from the lame gait. The pace decreased with

the 50 lb. load, although a stop watch was carried and effort was made to keep up the output of work. The method has decided practical advantages in the ease with which the load can be taken up from an ordinary table, and in the freedom of one arm.

TABLE IX.—*Mode* 5. *Carrying on the hip. Average net values.*

Load. lbs.	CO_2 per min.	O_2 per min.	Cal. per min.	Hor. kgm. per min.	O_2 per hor. kgm.	Gr. cals. per hor.kgm.
20	448	574	2·74	4726	·121	·58
30	532	657	3·16	4927	·133	·64
40	548	694	3·32	5124	·135	·65
50	566	725	3·46	5135	·141	·67

Approximate estimate of industrial output and physiological cost by Mode 5.

Load. lbs.	Material transported 100 yards in 1 hour. lbs.	Physiological cost in excess of cost of walking at same pace. Calories per hour.
40	920	78·8
50	1110	87·2

Mode 6.

In these experiments the load was carried in a rucksack, i.e. fairly low down on the back. The balance on the arches of the feet was recovered by bending the torso forward from the waist, and the arms dropped forward a little. The legs were free, and the movement of the chest was not appreciably restricted. For so short a period as one hour's work, no special fatigue would be expected, and none was occasioned, though pace was lost appreciably with the 50 lb. load.

The experimental data show, however, a cost unexpectedly high. Possibly the universally recognised value of knapsack-carrying depends more on the freedom allowed to arms and legs than on its metabolic cheapness relative to other methods. But in any case, these experiments are not comparable with any on the carriage of military equipment, for the soldier's load is balanced, not concentrated at one point of the back. The walker or climber, who may carry a fairly heavy rucksack with little to balance it in front of the body, commonly uses a stick, which, mechanically, alters the whole situation. The experiments were carried out with the object of seeing to what extent a mere displacement of the body affected metabolic costs. It is not a method of transportation likely to be applicable to women in industry.

There was no "empty" to be carried in unloaded circuits. The relatively high output of work with the 20 lb. load was due to a temporary rise of body weight.

TABLE X.—*Mode* 6. *Carrying in rucksack. Average net value.*

Load. lbs.	CO_2 per min.	O_2 per min.	Cal per min.	Hor. kgm. per min.	O_2 per hor. kgm.	Gr. cals. per hor.kgm.
20	421	561	2·66	4753	·118	·56
30	435	573	2·72	4792	·120	·57
40	474	608	2·90	5076	·120	·57
50	574	700	3·38	5117	·137	·66

Approximate estimate of industrial output and physiological cost by Mode 6.

Load. lbs.	Material transported 100 yards in 1 hour. lbs.	Physiological cost in excess of cost of walking at same pace. Calories per hour.
40	920	53·6
50	1125	82·1

Mode 7.

By the use of a shoulder-yoke, the advantages of retaining erect carriage and normal gait can be combined with an almost complete absence of such local discomforts as make bundle-carrying (Mode 3) distressing for long periods of work or for heavy loads. The yoke is of such width that pails suspended on chains hang clear of the legs, and the pails are adjusted to such a level that, when the arms are held easily with slightly flexed elbows, their handles can be steadied with the fingers. Theoretically, the plumb-lines of the chains should be in one line with the body's centre of gravity. The yoke used in these experiments did not quite fit the subject, putting the weight an inch or so too far back, so that the head and shoulders were slightly bent forward to right the balance. The displacement was, however, very small compared with that in other modes of carriage. The subject found no difficulty with the 60 lb. load, and could undoubtedly have carried much greater weights without distress. One disadvantage of the method is the weight of the yoke itself and of the pails, or suspended trays; this means that for a given inclusive load on the worker, a smaller quantity of actual material is transported than if no such appliance were used. A further practical disadvantage might be the width of gangway required for the manoeuvring of loads so suspended.

In the experiments, the yoke with its chains, weighing 4·8 lb. (the same weight as the empty tray) was carried in the unloaded circuits.

This set of observations, unlike the rest, had to be carried out in the afternoon. The effect of this may have been to give a rather lower scale of readings than would have been obtained in the morning; but several of the morning experiments were duplicated in the evening to get an approximate correction factor, and it was found that by calculating even with the extreme possible correction, the yoke still appeared very considerably the cheapest mode of carrying 40, 50, and 60 lb.*

TABLE XI.—*Mode 7. Carrying with a shoulder-yoke. Average net values.*

Load.	CO_2 per min.	O_2 per min.	Cal. per min.	Hor. kgm. per min.	O_2 per hor. kgm.	Gr. cals. per hor. kgm.
20	348	400	1·95	4865	·082	·40
30	356	440	2.12	4960	·089	·43
40	403	486	2·35	5157	·094	·46
50	428	516	2·50	5293	·097	·47
60	473	531	2·61	5483	·097	·48

Approximate estimate of industrial output and physiological cost by Mode 7.

Load.	Material transported 100 yards in 1 hour.	Physiological cost in excess of cost of walking at same pace.
lbs.	lbs.	Calories per hour.
40	720	20·5
50	950	29·2
60	1180	35·9

Mode 8.

Theoretically, the carriage of the load upon the head might be expected to be physiologically economical, since the additional weight is in the line of the natural centre of gravity, and the need of accurate balance should suppress all extraneous movements. The question of extraneous movements has not hitherto been discussed, though it has been indicated that they are a factor tending to raise the scale of costs of all types of work to the untrained worker. Such movements are non-purposive and

* The point at issue here involves the causes and effects of various levels of the respiratory quotient, an obscure question chiefly of technical interest, so that discussion of it is omitted.

are due to bad co-ordination, perhaps through lack of practice, or fatigue, or inattention, or bad working habits contracted through lack of intelligent analysis of the movement required, or perhaps through mere exuberance.

In the head-carrying experiments, non-purposive movements may have been eliminated, but a very large number of special muscular contractions and relaxations seem to have been required to maintain the balance of the load in so unfamiliar a position. More than any other, this mode of carriage requires practice, not merely for the development of the appropriate muscles, but for acquiring co-ordination of the existing muscular equipment.

The subject was not consciously anxious, and was not unduly fatigued, but she was aware of a high degree of muscular tension throughout the body. There was also some interference with normal breathing, for one hand was required to balance even the 20 lb. load, while the 40 and 50 lb. loads were steadied by the hands, on both sides. The loads were carried distributed over the same tray as was used in the other experiments. They were much more difficult to balance than the same loads would have been if more nearly concentrated in a point, e.g. carried as water in a jar, or as a mass of mortar on a small board. The mechanical difficulty of the task was possibly the predominant cause of the rather high scale of costs which was recorded. It is proposed to check these experiments by a further series in which the load is better disposed.

TABLE XII.—*Mode* 8. *Carrying on the head. Average net values.*

Load.	CO_2 per min.	O_2 per min.	Cal. per min.	Hor. kgm. per min.	O_2 per hor. kgm.	Gr. cals. per hor. kgm.
20	406	527	2·51	4821	·109	·52
30	449	575	2·75	4942	·116	·56
40	482	626	2·98	5168	·121	·58
50	561	692	3·30	5267	·131	·63

Approximate estimate of industrial output and physiological cost by Mode 8.

Load.	Material transported 100 yards in 1 hour.	Physiological cost in excess of cost of walking at same pace.
lbs.	lbs.	Calories per hour.
40	920	58·3
50	1150	79·2

Comparison of the Physiological Costs of the Different Modes of Carriage.

It is desirable not only to determine the relative metabolic costs of different methods of carrying any given load, but also,

if possible, to find objective evidence of reasons for the variations which appear. Examination of certain measurable physiological effects one by one might be expected to throw some light on the underlying causes. Hence we may compare not only the oxygen consumption, but also the ventilation rates, the percentage increases of respiration and pulse rates, and of the blood pressure, over their respective resting values.

The Ventilation Rate.

The "ventilation" of the lungs is the volume expired in a given time, in this case, per minute. In the table, the net rate of working ventilation is given, i.e. the pre-work resting value is deducted.

TABLE XIII.—*Comparative Ventilation Rates. Net Litres N.T.P. Dry.*

Load. lbs.	20	30	40	50	60
Mode 1	11·26	12·64	14·80	17·44	—
2	11·57*	12·98	17·11*	18·33*	—
3	10·83	10·35*	12·63*	14·28	—
4	11·88	13·37*	12·63*	13·01*	15·81*
5	11·92	14·94	15·10	14·61	—
6	11·57	12·37	13·54	14·81	—
7	10·85	10·15	12·08	11·91	12·81
8	11·40	11·55	12·60	14·37	—

These figures seem to afford some indication of the degree of interference with respiratory function. *Mode* 7 (the yoke) shows a very low rate throughout, and *Mode* 3 (bundles) is also low for the 20 and 30 lb. loads. The highest rates occur in the two series of tray-carrying in front of the body, where chest-fixation is greatest.

The Increase of Respiration Rate.

The percentage increase of the working respiration rate over the pre-work lying rate has been worked out. The "working" rate was counted standing, during the first half-minute after the conclusion of the hour's work. The increases found were very variable and did not correlate with those in the last table. Respiration rates would seem to be of small value as indications of fine distinctions in the effects of muscular work.

Pulse Rate and Blood Pressure.

The pulse-rates are calculated as increases per cent. over the pre-work lying-down rate (after 30 minutes' rest). The "working" pulse-rate was counted between the fiftieth and the eightieth second after the cessation of the working hour.

* *Average* pre-work deducted.

TABLE XIV.—*Pulse Rate. Percentage Increases over Lying at Rest.*

Load. lbs.	20	30	40	50	60
	%	%	%	%	%
Mode 2	33	22	40	35	—
5	56	53	56	64	—
6	30	47	41	52	—
7	20	20	33	32	27
8	26	45	80	78	—

The yoke method (No. 7) again gives a low increase throughout; the hip-carrying (No. 5) shows a uniformly fairly high rise. The pulse rates for carrying on the head (No. 8) (124 p.m. for 40 lb.; 121 p.m. for 50 lb.) are the highest recorded on this subject for work of this type.

An attempt was also made to determine the influence of the mode and load on the blood pressure, but owing to the rapid fall on the cessation of work and the difficulty of getting the post-work determinations made in each experiment exactly at the same number of seconds afterwards, the results obtained were rather unreliable, and have been omitted.

The Oxygen Consumption.

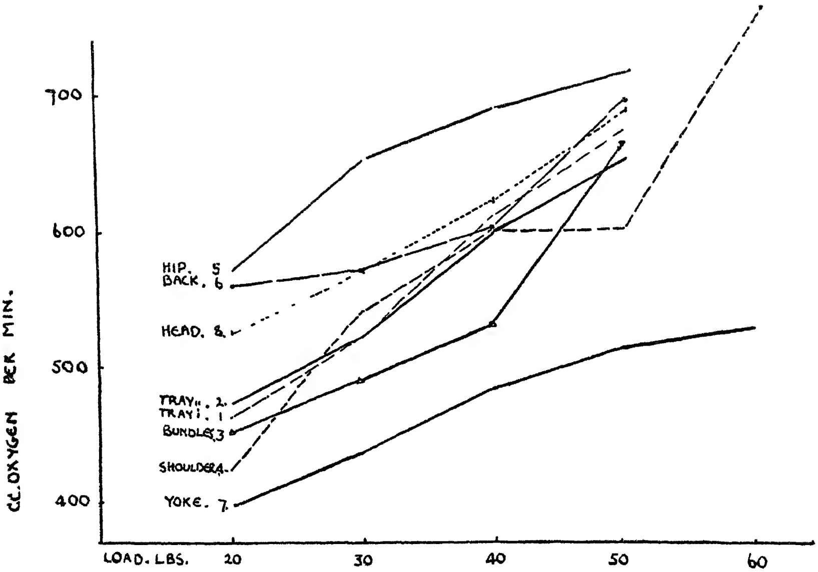
Metabolic costs are commonly expressed either in oxygen consumption or in caloric expenditure, per minute. In the previous tables, both sets of data are given. For the sake of lucidity, only the oxygen consumptions are given in the two following comparative tables.

The net oxygen consumption per minute (i.e. the lying pre-work requirement being deducted) gives perhaps the clearest picture obtainable of the relative physical effort required to carry the several loads in different ways (Table XVI and Graph I). But since the experimental ideal of exactly equal performances of work for each load, by all modes of carriage, was imperfectly realised, it is necessary to check these data by considering the cost of work per horizontal kilogrammetre (Table XVII and Graph II). In this table the modes are arranged in order of decreasing cost for each load.

TABLE XVI.—*Comparative Cost of Modes of Carriage. O_2 consumption per minute, net.*

Load. lbs.	20	30	40	50	60
Mode 1	464	522	613	675	—
2	473*	522	604*	656*	—
3	455	492*	534*	667	—
4	428	547*	609*	608*	778*
5	574	657	694	725	—
6	561	573	608	700	—
7	400	440	486	516	531
8	527	575	626	692	—

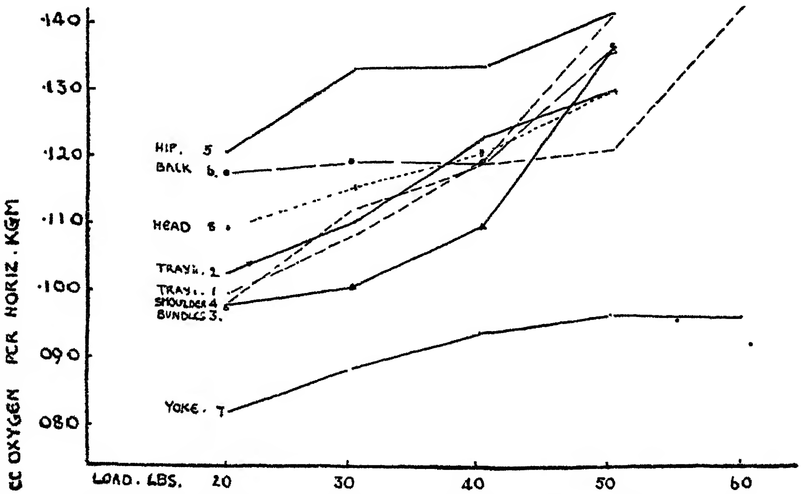
**Average* pre-work deducted.



Graph I. Comparative costs of eight modes of carriage: Oxygen consumption per minute. Net.

TABLE XVII.—*Decreasing Costs. c.c. Oxygen per horizontal kilogrammetre. Net values.*

20 lbs.		30 lbs.		40 lbs.		50 lbs.		60 lbs.	
Hip	·121	Hip	·133	Hip	·135	Tray I. ..	·142	Shoulder ..	·144
Back ..	·118	Back ..	·120	Tray II. ..	·122	Hip	·141	—	
Head	·109	Head ..	·116	Head ..	·121	Back }	·137	—	
Tray II. ..	·103	Shoulder ..	·113	Tray I. }		Bundles }		—	
Tray I. ..	·100	Tray II. ..	·111	Back }	·120	Tray II. }	·131	—	
Bundles ..	·099	Tray I. ..	·109	Shoulder }		Head }		—	
Shoulder ..	·090	Bundles ..	·101	Bundles ..	·110	Shoulder ..	·122	—	
Yoke ..	·082	Yoke ..	·089	Yoke ..	·094	Yoke ..	·097	Yoke ..	·097



Graph II. Comparative costs of eight modes of carriage. Oxygen per horizontal kilogrammetre of work performed. Net.

Interpretation of the Results.

By the consideration of, for instance, the curves of Graph II, some summary may be made of the findings of this investigation.

The metabolic costs of yoke-carrying, Mode VII (even allowing for the extreme correction, suggested on page 18 above), are conspicuously low; the tables of ventilation, pulse and blood-pressure changes show that this method involves less physiological disturbance than any other, and the subjective estimate strongly confirms this. The shape of this curve in the two graphs should be noted; its relative flatness indicates that even at 60 lb. load, the point has not been reached where physiological costs rise more than proportionately to the increment of work-performance.

On this point, the curve next above it in the graph, that for bundle-carrying, offers a contrast. The very steep rise of cost at 50 lb. load is unquestionably due to severe local strain, and the same factor accounts for the abrupt rise in the curve of Mode I (Tray I). The relatively low costs of the 20 lb. and 30 lb. bundles are probably due to the same causes which make yoke-carrying pre-eminently satisfactory, viz.: the erect position and the free gait.

The next curve, for shoulder-carrying, has a break at 50 lb. which should probably be smoothed out (see page 15). The curve would then rise fairly equally, indicating (as the subjective impressions confirm) that the method does not become markedly "inefficient" for any of the loads tested. There are, unfortunately, no data for pulse-rate, etc., in this case. Though apparently not physiologically unsatisfactory, the method is much more costly than carrying with the yoke. It is suggested that this is due in the main to the displacement of the body necessary to bring the centre of gravity over the feet.

The curves for carrying in trays and on the head require little comment. The high level of the whole curve for head-carrying may be attributable to chest-fixation and undue muscular tension. The critical point in the case of Tray I has been pointed out above.

The same critical point, marking 40 lb. as the maximum economic load, occurs also in the curve for rucksack-carrying, but here the disproportionately greater cost of 50 lb. is not due to local fatigue. No explanation of this "efficiency point" offers itself from the data collected. The high position of the rucksack curve as a whole may be connected with the pronounced stoop which compensates the dorsal load.

In the curve for carrying on the hip, there is again some indication that an efficiency point is reached with a load round about 35 per cent. of the body weight, and the pulse rates here correlate with the metabolic data. The costliness of this mode of transportation would seem to depend chiefly on the conjunction

of two conditions: the curvature of the body, for the effect of which there is already some evidence, and the virtual immobilisation of the knee and ankle joints of one leg, causing a very lame gait.

It is evident that the interaction of the four factors—local strain, posture, gait, and chest freedom—is complex, and is moreover different for different loads. It is not possible to state definitely which is the most important, and there may be other factors involved. But it is at least apparent that the hip-carrying, which has all four disadvantages, is metabolically a more costly method of transporting material than any other tested. It is also clear that yoke-carrying, where these disabling factors are absent or reduced to a minimum, is physiologically inexpensive.

The curves which lie close together in the middle of the graphs might be found in rather different positions, relative to one another, in another subject or in the same subject after practice. The contrast, however, between the highest and lowest curves is so great as to suggest that the physical factors discussed will affect physiological costs permanently.

Mechanical Efficiency and the Industrial Effectiveness of the Human Machine.

This paper deals primarily with the relative desirability of different methods of carrying loads; incidentally with their weight; and not at all with the question of a maximum "safe" load.

Any organisation or inanimate machine, is, of course, most efficient when running at the full capacity for which it is designed. Similarly, on various grounds, it is undesirable that the "human machine" should work much below its capacity. Suppose the case that a woman is, in every way, perfectly capable of transporting material in 60 lb. loads, over a given distance, at a given pace, for so many hours per day. If she were to carry it in 30 lb. loads at the same pace, the wages bill would in effect be doubled, while the woman herself would not benefit proportionately. Her physiological costs would be much less than halved, while mental tedium might well be increased, for she would lose that element of pleasure which, in health, is commonly found in doing "what takes a bit of doing". If, on the other hand, she were required to complete the same total of external work in the same time, in 30 lb. loads, by doubling the pace, the immediate result to the management might be much the same, but the metabolic costs and nervous strain to the worker would be dangerously increased.

A worker's habitual speed for continuous labour may not actually be his optimum, but it will almost certainly be found at some point within the rather narrow range of speeds "physiologically efficient" for such work. At higher speeds steady work cannot be maintained without rest-pauses. Finally there will be an upper limiting speed, perhaps double that which is habitual, which can only be maintained for a few minutes.

The broad relations of speed and load to the effective working of the human machine may easily be seen by anyone who cares to experiment on, for example, Cathcart's hand-lever ergometer. Suppose that for a certain load, 100 movements of the levers per minute is found a satisfactory speed, one that can be kept up for hours. If the speed be increased to 200 strokes per minute, the experimenter may find difficulty in lasting out ten minutes. If a metabolic determination is made, the costs per kilogrammetre may be, or appear to be, lower; but this may be due to the fact that the body in such a case runs up an oxygen debt, and to measure the whole oxidation demanded by the exertion it would be necessary to study the more or less prolonged recovery period. Even should complete measurement still show a high mechanical efficiency, there remains the practical point that such exertion can only be maintained for a few minutes. Suppose now that the levers are worked at a rate of 50 strokes per minute; the physiological costs will not be halved, and therefore the cost per kilogrammetre will be higher.

If now the speed be kept constant, and the load on the ergometer-brake varied, much the same thing appears. Any subject has a small range of loads, suitable to his physique, with which at a normal speed, say 100 strokes per minute, he can keep up his optimum output of work for some hours. If the load is doubled, the cost per kilogrammetre may be somewhat reduced, and so the mechanical efficiency raised, but not by much; and in extreme cases, the opposite may be found. In any case, the tax on the organism is such that the work cannot be continued for anything like one-half the time possible with the normal load, and so the total output of external work is less. If the normal load now is halved, the physiological costs are reduced by much less than half.

Loads too low are inefficient in the mechanical sense; loads too high may be mechanically efficient, if the time (endurance) element is disregarded, but they make strongly against the industrial effectiveness of the human body by reducing the period for which the exertion can be maintained, and by leaving after-effects of fatigue.

Applying this to the experiments under consideration, Table XVI, Mode 7, may be taken to illustrate the points at issue. The cost rate per minute to the body of carrying 20 lb. is 400 cc. of oxygen; to move 40 lb. at the same pace costs much less than 2×400 cc.; and to move 60 lb. at the same pace costs much less than 3×400 cc. Therefore, it seems, on the ground of mechanical efficiency, that it is preferable to carry 60 lb.; and this is, of course, true (with the proviso discussed below) for simple reasons. In each case the body weight has also to be carried, and the greater the external load, the greater is the productive result of this physiological exertion. In other words, it is clear that, by reducing the load by one-half or two-thirds, and by making therefore two or three journeys

instead of one, the maintenance charges of the working organism for two or three minutes instead of one minute, are bound to outweigh the economy of the lower cost per kilogrammetre of the lower loads.

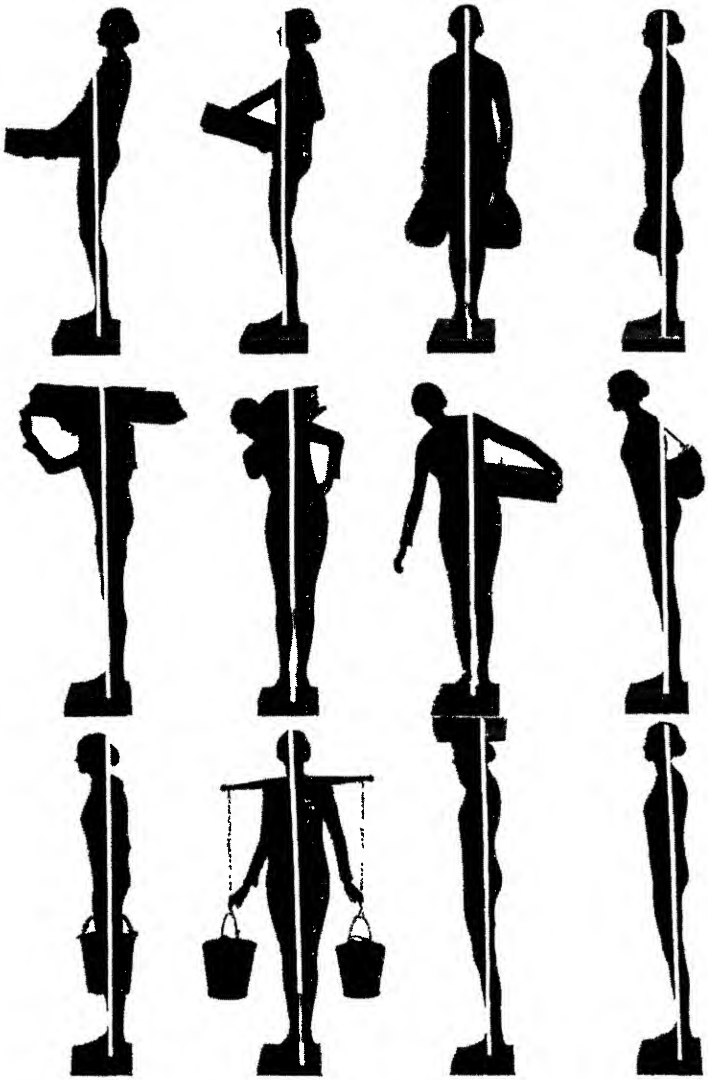
The proviso is, however, all-important—that the capacity of the organism for continued effort must not be impaired. The " lazy man's load " may break his back, and the pursuit of mere mechanical efficiency may lead to the advocacy of a load (or a speed) which is physiologically excessive and, therefore, in the long run, industrially ineffective.

The tables given show that the cost of work *per horizontal kilogrammetre* rises absolutely with the load carried. Even the light loads, except perhaps on the yoke, are not carried so efficiently as so much body weight, and in some cases each addition to the load produces a very considerable increase in the cost of every kilogrammetre of the work. This has been very generally found in experimental work of this kind, and it does not contradict the principle that it is more economical to work up to one's capacity than below it, as common sense urges, and scientific measurement demonstrates (Table XVI).

What is of great importance is not this absolute rise of costs per horizontal kilogrammetre, but the *relative* rise which is seen in Graph II, where some cost-curves, between the 40 and 50 lb. loads, turn sharply upwards, while the work-curve rises equally, or in some cases flattens down somewhat from the subject's inability to keep up the pace. (The work-curves are not drawn in Graph II, but see the data in Tables V to XII).

Where this relative increase of costs sets in, the physiological efficiency point may probably be located. Beyond this, the cost to the organism of every additional kilogrammetre of work is disproportionately heavy, and it is significant that at this point also the output of work was in some cases unavoidably reduced. The rough computations of output and costs for an hour's work, at the foot of Tables V to XII are worth examining.

From the tables and figures, it appears that the efficiency point was not exceeded with the 60 lb. load, possibly in shoulder-carrying (Mode 4), certainly in yoke-carrying (Mode 7). But in most of the other modes, the efficiency point may be placed at 40 lb. This is not to say that women in workshops should be forbidden ever to carry more than 40 lb. by any of these methods, but it does mean that they are physiologically very costly for prolonged work with heavier loads. More observations are needed, both from laboratory and factory, before it will be possible to state, even in general terms, what is the economic load for normal women in industry. These experiments suggest that the continuous carrying of a load exceeding about 35 % of body-weight, if so disposed as to disturb normal poise and movement, is likely to cause rapid impairment of working capacity.



Top row : (1) Mode 1 ; (2) Mode 2 ; (3) & (4) Mode 3.
Second row : (1) & (2) Mode 4 , (3) Mode 5 ; (4) Mode 6.
Third row . (1) & (2) Mode 7 ; (3) Mode 8 ; (4) Normal standing.

When the experimental work was completed, a number of shadow-photographs were taken, to verify the analyses made of the adaptations of the bodily posture to the load in its various positions. Photography of the work in progress being impracticable without special equipment, the standing position was taken, the load being in every case 40 lb. These pictures show with some exactness the degree of body-displacement necessary to maintain the normal balance on the arch of the feet with minimal muscular tension. There is nothing accidental about the posture taken up: the optimum position could be gauged to a nicety. Several of the positions were taken in duplicate and triplicate on different occasions, and the versions of each mode are hardly distinguishable from each other. In the profile pictures, a line tentatively suggesting the plane of the centre of gravity is drawn through a point about one inch in front of the external malleolus.

The silhouettes illustrate most of the circumstances, favourable and unfavourable, which have been discussed as affecting the cost of the different modes of carriage.

It is a great pleasure to acknowledge my gratitude to Professor Cathcart for help of every sort throughout this work; and to Mrs. Cathcart for her kindness in taking the photographs.

For the first four sets of experiments, Dr. E. M. Hewitt is equally responsible with myself, and they formed part of the material of a report published in the Chief Inspector of Factories' Annual Report, 1923. It is by her permission, and that of the Home Office, that these data are here incorporated with the rest of the work.

My thanks are also due to Dr. Gillespie and Dr. Wishart, who very kindly helped me by acting as observers in some of the later experiments.

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December, 1923.

PART B.

THE INFLUENCE OF REST PAUSES AND CHANGES OF POSTURE ON THE CAPACITY FOR MUSCULAR WORK.

By H. M. VERNON, M.D.,

Investigator to the Board.

CONTENTS.

Introduction.

In industrial work, especially if it is of a strenuous physical character, it is sometimes found desirable to introduce rest pauses at regular intervals between bouts of hard labour, but we have no information as to what constitute the most suitable systems of rest pauses under various conditions. It is very difficult to investigate the subject under industrial conditions, so an attempt was made to do so in the laboratory. Scarcely any laboratory experiments on the effect of rest pauses have been made hitherto, partly because most investigations on muscular work have been made by means of the well-known ergograph of Mosso (1890), or instruments of similar type. In these instruments a heavy weight is raised by means of the middle finger of one hand at regular intervals, and usually the capacity for raising the weight falls to zero in a minute or two. Muscular strength is very far from being exhausted, however, and it is only necessary to lighten the load in order to enable the finger to continue its contractions.

A great improvement on the principle of the ergograph was employed by Schenck (1900), who allowed a muscle of the hand (the *abductor indicis*) to work isometrically against a spring (Fick's apparatus). He found that when the muscle was alternately contracted for a second and relaxed for a second, the

strength of its contractions fell in three minutes to about two-thirds of their initial value, and then remained practically constant for as long as the experiment was continued, even if it lasted an hour. Hough (1901), who used an ergograph in which the middle finger pulled against a spring, obtained a similar result. He contracted the finger for half a second, and relaxed it for 1½ seconds, and he found that in a period of one to seven minutes the height of the contractions fell to a level which was 70 to 80 per cent. the height of the initial contraction. If the rhythm were altered, and the relaxation interval were increased from 1½ seconds to 9½ seconds, the contractions regained their initial height in about a minute. Hough found that the height of the "fatigue level" varied inversely with the rapidity of the rhythm of contraction.

It is evident that an ergograph which worked against a spring could be used for investigating the effect of rest pauses on the capacity for muscular work, though it is uncertain how far the effects produced in two forearm muscles could be held to apply to the much larger groups of muscles usually employed in industrial work. No such observations appear to have been made hitherto, and the only record I can find of rest pause experiments are those made by Wallrich and Dawson (1921) with a bicycle ergometer. These investigators determined the number of revolutions made in an hour (*a*) when the bicycle was ridden continuously; (*b*) when it was ridden for ten-minute periods, followed by five minutes' rest. One subject, when working continuously against a 16-kilogram break, at first made 2,050 to 4,078 revolutions, and after several days' practice, 6,409 to 6,895 revolutions. When rest pauses were interposed, however, he made 3,946 to 4,752 revolutions at first, or distinctly more than in the absence of rest pauses, but after several days' practice he increased his revolutions only to 5,176 to 5,204. Hence the rest pauses were disadvantageous to him when in training, but advantageous when he was untrained.

Much light on the fundamental principles which determine the need of rest pauses is afforded by the recent investigations of A. V. Hill and his colleagues. As already mentioned, both Schenck and Hough found that after a brief initial period their muscular contractions remained at a steady level, and Fletcher (1902) found that if a frog's gastrocnemius muscle were excited at regular intervals, it likewise, after an initial fall of contraction height, showed a prolonged period of nearly steady contractions, provided that it was kept in an atmosphere of oxygen. Hill and Lupton (1923) found that when a trained subject ran at a steady pace, his oxygen consumption rapidly rose, and after about 2½ minutes reached a steady level. This was maintained even when the running was continued for as long as 27 minutes. The magnitude of the oxygen consumption increased with the speed of the runner up to a certain point, but beyond that it showed very little further rise, whatever the speed. The reason was that the maximum intake of the body had been reached, this

maximum depending on the capacity of the lungs and circulatory system, and other factors. However, the " oxygen requirement " of the runner's muscles increase rapidly with increasing speed, and in consequence they get more and more into " oxygen debt." The extent of the debt permissible is strictly limited, and it can be paid only by taking an actual rest from work, or by slowing down the rate of work. That is to say, muscular work is divisible into two distinct classes :—(*a*) that which is performed with such vigour that the body runs into an oxygen debt which absolutely necessitates the taking of rest pauses, and (*b*) that which is performed with a less degree of vigour, and on that account permits the attainment of the so-called " steady state." This state can in theory be maintained indefinitely without the necessity of rest pauses, and its existence depends on the fact that the rate of lactic acid production in the body is balanced by the rate of its oxidation and removal, so its concentration in the muscles remains constant. When the body is in oxygen debt, however, the lactic acid accumulates in the muscles, till it (or rather the hydrogen ion concentration) reaches a concentration sufficient to inhibit further activity.

All of the muscular work described in the present report is of the second class : i.e. it was of such a character as to permit the attainment of a " steady state." It is important that this condition should not be overstepped in industrial work, for its existence implies that the breakdown in the muscles is never greater than the maximum rate of recovery. In some forms of industrial work it may occasionally be necessary for the worker to exert himself so strenuously for a short time as to run into oxygen debt, but there can be little doubt that such work is extremely fatiguing, and should be avoided whenever possible.

The Dynamometer Employed.

In my own experiments, of which a preliminary account was published in 1922, I employed throughout a dynamometer which tested the strength of the back and arm muscles. Its form can be gathered from Figs. 1 and 2. It has the merit of being portable, and it is unnecessary to fasten it to the floor when in use, as the subject stands on the cross-bar of the **T**-piece and pulls against it. In the recording experiments, however, I found it best to screw it to the floor, in order to prevent the risk of shifting. The spring balance of the dynamometer registered up to 500 lb., and it was provided with an indicator hand, as well as the hand connected with the spring. In the first series of experiments, the chain attached to the handle was connected with the spring balance by means of a wire cable which passed under a pulley wheel, and as the pulley was on ball bearings, the loss of power from friction was very small. The cable wore out rather quickly and had to be renewed, so in the second series of experiments it was replaced by a bicycle chain. This proved quite satisfactory.

To face page 30.

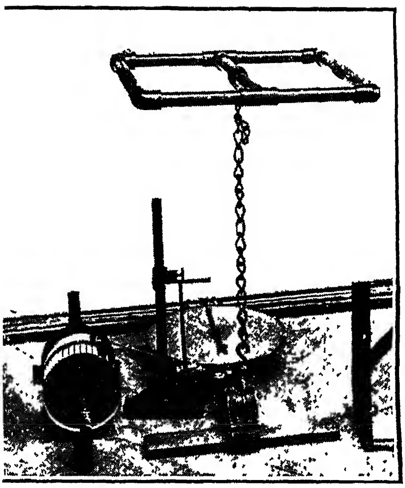

Fig. 1. Dynamometer showing frame handle

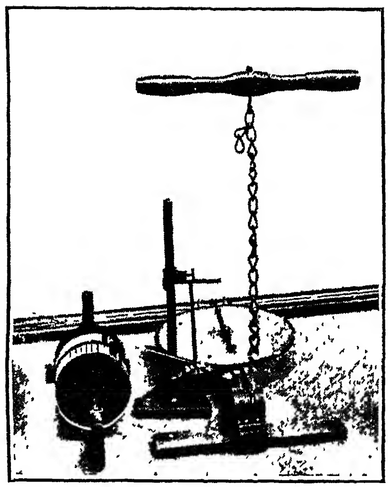

Fig 2 Dynamometer showing ordinary form of handle.

The recording lever consisted of a long steel rod with one end bent and sliding in a small grooved plate of brass fixed to the iron connecting arm of the spring balance. As this arm moved straight backwards and forwards when the handle was pulled, whilst the bent recording rod moved in a circle, it was impossible to obtain a perfect fit between the two, but the very small amount of play necessary can be gathered from the records reproduced below. The position of the steel rod could be altered so as to obtain any magnification of movement desired, but as a rule the magnification was 3·5 to 4·0-fold in the first series of experiments, and 2·8-fold in the second series. The spring balance arm moved only 11 m.m. when subjected to a 200 lb. pull, so the muscular contractions were practically isometric. The dynamometer was provided with the two forms of handle shown in Figs. 1 and 2. The " frame " handle is theoretically the best, as it enables the subject, who gets inside the frame, to stand vertically over the resistance against which he is pulling, but it has several disadvantages. In order to be certain that it would stand a pull of 500 lb., it was made very substantially, and it weighed 11¾ lb. For ordinary purposes it would have been strong enough if of half this weight, but even then it would have to be supported whenever a rest pause was being taken, whereas the ordinary form of handle, which weighed only 10½ oz., or 20½ oz. with the chain, could easily be held in one hand without causing fatigue.

The dynamometer itself was made to my design by Mr. S. W. Bush, of the Oxford Scientific Instrument Works, and it proved quite reliable. It can be pulled continuously for an hour or more without causing any pain in the hands, once they have got hardened by practice. This is a great advantage over hand-grip dynamometers, which are apt to cause pain after a short period of use.

The Maximum Strength of Pull.

Before starting the rest pause experiments, I made a series of control experiments with the dynamometer for several months, more especially to find out the conditions under which a maximum pull could be exerted. By fastening the handle to different links of the chain, I could determine my pull at different heights above floor level, from 10 in. and upwards. In order to get nearer than this to the level on which I was standing, I stood on bricks placed on either side of the pulley wheel, but I could not conveniently manage to pull when nearer than 2·0 in. with the frame, and 2·9 in. with the ordinary handle. The pulls were made gradually without any jerk, and were maintained several seconds at the maximum tension.

After a series of observations in which I made the maximum pulls every day for a month, I made a series of 15 pulls on each of six days with the frame at different heights above floor level. An interval of about four minutes was allowed to elapse between each pull, and on alternate days I started at the full

length of chain or at floor level, and worked steadily to the opposite position. Owing to fatigue I weakened slightly during the cycle, and my last four pulls were, on an average, 3·8 per cent. smaller than my first four. In the next series of frame

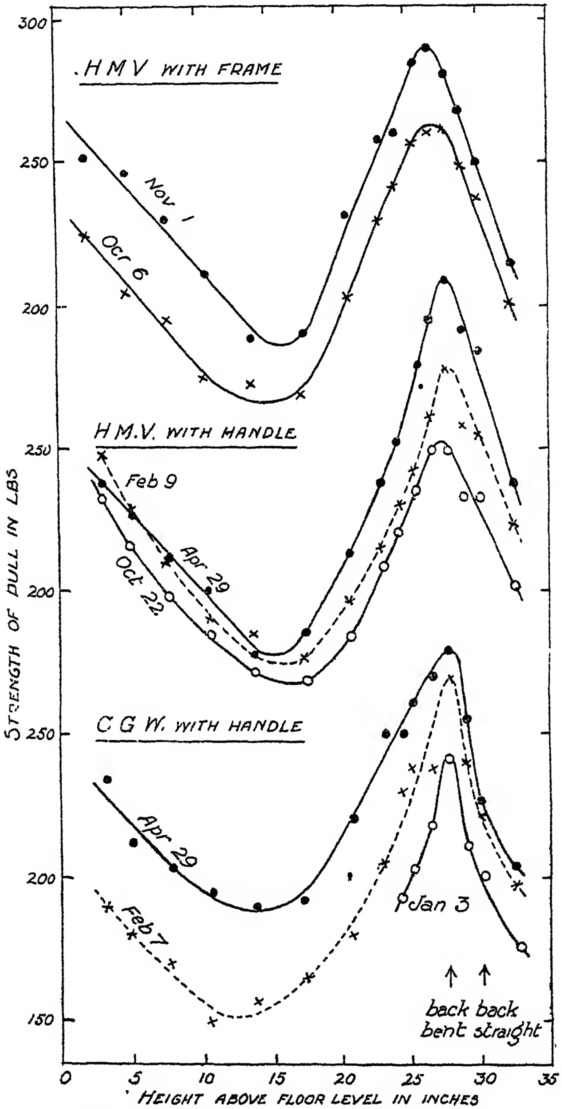

Fig. 3. Maximum pull at various heights above floor level.

experiments, made about four weeks later, I allowed an interval of about 15 minutes to elapse between each pull. Then my last four pulls were 3·6 per cent. smaller than my first four.

The mean results of the frame experiments are plotted out in the upper part of Fig. 3, whilst the actual strengths of pull at

the salient points are recorded in Table I. It will be seen that my pull was fairly considerable just above floor level, and then fell gradually till it reached a minimum when at a height of 17·2 in. From this point it rapidly increased, and reached its

TABLE I.—*Average pull in lb. at different heights above floor level.*

Height above floor.	H.M.V. with frame.		H.M.V. with handle.			C.G.W. with handle.		
	Oct. 6	Nov. 1	Oct. 22	Feb. 9	Apr. 29	Jan. 3	Feb. 7	Apr. 29
2·0 (or 2·9) in.	226	252	232	247	238	—	189	234
17·2 (or 13·7) in.	168	189	168	177	178	—	150	189
27·7 in. ..	262	291	249	277	309	242	269	280
32·4 in. ..	202	216	202	224	238	176	198	204
Mean of pulls at all heights	219	244	212	227	243	—	202	229
Mean variation (as per cent.)	5·4	4·1	4·4	3·7	4·0	—	3·7	2·2

maximum at a height of 27·7 in. above floor level. At greater heights still it rapidly weakened again. The results obtained about 1st November resemble those obtained about 6th October, except that my strength of pull (the average of the 15 sets of observations at all heights) was 25 lb. greater.

The data obtained in the three sets of handle experiments are recorded in the middle of Fig. 3. The curves resemble those obtained with the frame, only they show a relatively smaller pull. Thus the series made about 22nd October shows a mean pull of 212 lb., which is 8 per cent. less than the average of the preceding and succeeding frame series. This represents the loss of power produced by pulling in the mechanically imperfect position necessitated by the use of the handle, and the further the handle is held from the body, the less the strength of pull. For instance, at a distance of 2 in. in front of the legs the strength of pull was 27 per cent. less than with the frame; at a distance of 4 in. it was 37 per cent. less, and at one of 6 in., 45 per cent. less. These results are the means of six sets of observations in each case.

The figures recorded in Table I show that between 22nd October and 29th April my mean pull improved from 212 lb. to 243 lb., owing to practice. The other figures recorded show that Mr. C. G. Warner, who repeated most of the dynamometer experiments made by myself, showed a greater improvement still. His handle experiments are plotted out in the lower part of Fig. 3, and it will be seen that though the curves have a general resemblance to mine, they do not correspond closely.

The individual observations on the strength of pull showed a good deal of variation, and the extent of this variation is recorded as the "mean variation," reckoned as a percentage on the mean pull. It varied from 3·7 to 5·4 per cent. in my own series of observations, and from 2·2 to 3·7 per cent. in those of C. G. W. This percentage variation remained nearly constant at all heights of pull, whether at floor level or at the minimum or maximum positions.

The existence of a weak spot in one's pulling or lifting capacity is a fact of practical importance which, though known to many workmen, has not hitherto been subjected to accurate measurement, so far as I am aware. Supposing that I myself, for instance, wished to lift a weight of 200 lb. from the floor to a bench 2 ft. to 2½ ft. in height. I should find that I could lift it up a short distance without much difficulty, and I should also have found, had I tried it, that I could easily raise it from the bench itself, but if I tried to raise it from the floor to the bench I should have to move it through a position at which my maximum lifting capacity was only 168 to 178 lb., and I should inevitably strain my muscles.

The capacity for pulling and lifting is dependent chiefly on the strength of the back and arm muscles, for the leg muscles always possess very much more strength than is required for the purpose. This is brought out rather strikingly by comparing the maximum strength of pull (with handle at optimum height above floor level) when standing on one leg and on both legs. When pulling on one leg it is necessary to steady oneself by touching the floor with the toe of the other foot, but practically no pressure need be exerted.

TABLE II.—*Relative strength of pull.*

Conditions.	H.M.V.	C.G.W.
Standing on both feet and using both hands	100	100
„ right foot „ „	98	96
„ left foot „ „	98	98
„ both feet „ right hand	55	49
„ „ „ left hand . .	52	54

It will be seen from Table II that if the strength of pull under ordinary conditions is taken as 100, it is only 2 to 4 per cent. less when standing on one leg. It seems to follow, therefore, that if a man, when trying to wheel a very heavy wheelbarrow, finds that he can manage to raise the handles from the ground, he ought to be able to move forward alternately on one foot and on the other without overstraining himself. Even in the weak spot region one leg is almost as good as two, for I found that at

15·8 in. above floor level I pulled exactly the same when standing on my left leg as I did on both legs, whilst on my right leg I pulled 2 per cent. less.

When we pulled the handle by means of one hand, which was used to grip the centre where the chain is fixed, we found that our strength was only slightly more than *half* as great as when both hands were used. I myself pulled 55 per cent. of my two-handed strength when using my right hand, and 52 per cent. of it when using my left, but C. G. W., who is left-handed, showed values of 49 and 54 per cent. respectively. The results recorded are the means of eight or ten sets of observations in each case.

Continuous Work at Different Rhythms.

In all the experiments to be described, the dynamometer was pulled by means of the handle, which was fixed either at the optimum height of 27·7 in. above floor level, or at a height of 30·0 in. At this latter height my maximum strength of pull was 7 per cent. less than at the former, whilst that of C. G. W. was 18 per cent. less, but the experiments were rather less tiring as the back was kept practically straight throughout, instead of in the slightly bent position necessitated by the shorter chain.

The handle was pulled synchronously with the ticks of a metronome, set throughout at a speed of half seconds. A maximum pull was made between the ticks 1 and 2, and relaxation lasted from tick 2, through tick 3, and continued till the beginning of the next one or more ticks, when another pull was made. As the result of a number of experiments in which the movement of the lever was recorded on a fast moving drum, I found in my own case that *on an average* the contraction phase lasted approximately half a second, whatever the duration of the relaxation phase. That is to say, with contraction for ticks 1, 2 and relaxation for tick 3, the actual contraction period lasted half a second, and the relaxation period one second. With contraction for ticks 1, 2, and relaxation for, e.g. ticks 3, 4, 5, 6, 7 and 8, the relaxation period lasted $3\frac{1}{2}$ seconds. In any given series of 20 consecutive pulls the duration of the actual contraction period was remarkably steady, but this was not the case in observations made on different days, or at different times on the same day, and I found that my actual contraction period varied between the extremes of 0·35 second and 0·63 second, whilst C. G. W. had a longer contraction period than I, and in the few observations made showed a period of 0·62 to 0·72 second.

It is quite impossible to ensure that the contraction period be maintained at all accurately at the half second desired, but the error introduced by the deviations is probably very small. Thus Hough (1901) found, by means of his spring ergograph, that when the middle finger was contracted for half a second

and relaxed for $1\frac{1}{2}$ seconds, the fatigue produced was just as great as when it was contracted for 1 second and relaxed for 1 second. Johansson (1901), who pulled an ergometer with both his arms, found that his CO_2 output increased 9 to 27 per cent. when the contractions lasted 2 seconds instead of 1 second. Again, Zoth (1906), who used a Mosso ergograph, found that the recovery time was about the same whether he kept his finger contracted for a full second each time, or made as rapid a contraction as possible.

My experimental results agree with those of Schenck and Hough in showing that a nearly steady level of contraction height is attained in a very few minutes, this height depending on the frequency of the contractions. In Fig. 4 is seen the effect of pulling once every 4 seconds for the first 6 minutes, and then of pulling once every $1\frac{1}{2}$ seconds. At first I pulled about 222 lb. each time, but in 4 minutes my pull had fallen to a nearly steady level of 202 lb. On changing to the more rapid rhythm, my pull fell to a level of 103 lb. in less than a minute, and remained steady at this value. In Fig. 5 the converse experiment is recorded. I started pulling once every $1\frac{1}{2}$ seconds, and it will be seen that I reached a nearly steady pull of 102 lb. in 5 minutes. After 10 minutes I changed to the other rhythm, and in $1\frac{1}{2}$ minutes I got to a steady pull of 207 lb. This was as great as the initial pull with the fast rhythm, but this result was partly due to the fact that I changed my posture (as described in the next section) during each $3\frac{1}{2}$ seconds' relaxation.

A number of other experiments were made in which the rhythm was changed in the middle, and they agreed in showing that a contraction height proportionate to the rhythm was attained in 1 or 2 minutes. This was less than the time needed for the attainment of an approximate level in the first half of each experiment, as this usually amounted to about 4 minutes. The shorter time observed in the middle contractions was due to the fact that some approach towards the attainment of the "steady state" (when there is a balance between the production of lactic acid on the one hand, and its oxidative removal on the other) had already been induced by the earlier contractions (cf. Hill and Lupton). I have stated that in all the experiments a "maximum" pull was made each time, but it was scarcely that. It was maximum with the mental proviso—of which one was scarcely conscious—that further maximum pulls were to be made immediately after. By an effort of the will a supra-maximal contraction could be made at any time, but this almost always had the effect of causing the next two or three contractions to be submaximal. This phenomenon is well shown in Fig. 5, where I made a supramaximal pull at 5 minutes from the start of the experiment, to simplify the subsequent

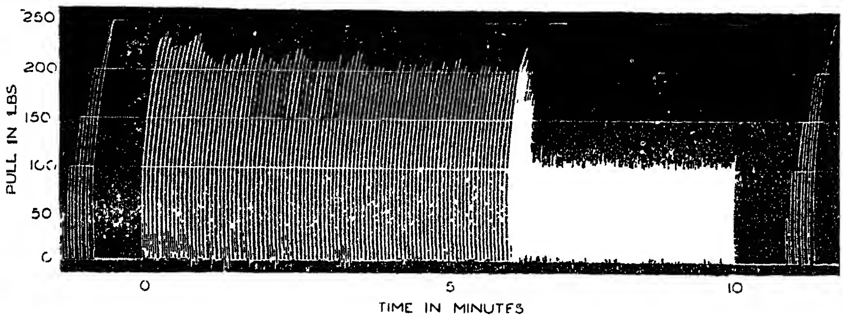

Fig 4 Contraction/Relaxation = $\frac{1}{2}$ sec /$3\frac{1}{2}$ secs for 6 min : then $\frac{1}{2}$/1 sec for 4 min

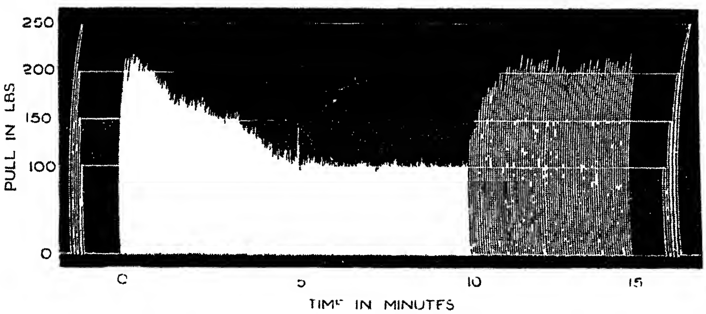

Fig 5. C/R = $\frac{1}{2}$/1 sec. for 10 min. : then $\frac{1}{2}$/$3\frac{1}{2}$ secs. for 5 min.

measuring of the record. Because of the irregularities caused by putting in such supramaximal contractions, I speedily gave them up.

The actual strength of pull was fairly steady for each rhythm, but in order to obtain really comparable results, I adopted several precautions. It has already been shewn that the average strength of pull increased considerably during the course of the investigation. In order to avoid this source of error, I mixed up my experiments at different rhythms and with different rest pauses interposed, so that all the groups of experiments might reap a similar advantage from my improvement in strength. Another source of error lay in the daily cycle of fatigue, but this proved to be a very small one. My usual custom was to make four experiments a day, viz., at about 9.0 a.m., 12 noon, 3 p.m., and 6 p.m. At first I made only 10-min. experiments, as I got too tired if they were of longer duration; but afterwards I made two 10-min. and two 20-min. experiments, or three 20-min. experiments, and longer ones still occasionally. Every kind of experiment was made on at least one occasion at each of the times mentioned, and on averaging 27 sets of 10-mins. experiments I obtained the following results :—

	9 a.m.	12 noon.	3 p.m.	6 p.m.
Average strength of pull during 1st minute..	206	205	201	203
Average strength of pull during 5th to 10th minute	147	148	151	149

It will be seen that though my initial strength of pull fell off about 4 lb. during the course of the day, my average strength during the 5th to 10th minutes (both inclusive) increased 4 lb. between 9 a.m. and 3 p.m., and then fell away slightly. In any case the diurnal variations are so small that for practical purposes they can be ignored.

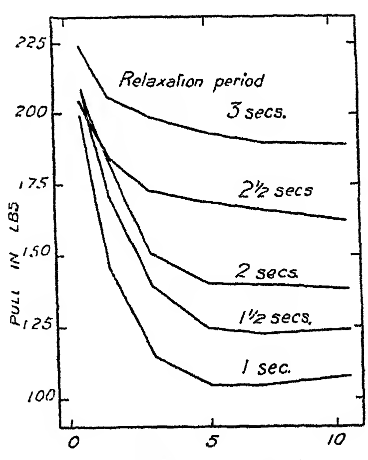

Fig. 6. The effect of duration of relaxation period on strength of pull.

In plotting out the experimental results, I averaged the strength of pull during the 1st, the 2nd, the 3rd and 4th, the 5th and 6th, the 7th and 8th, and the 9th and 10th minutes of experiment, and these average values are recorded in Fig 6. It will be seen that at each rhythm the contraction height reached a fairly steady level after about 4 minutes, so the mean level between the 5th and 10th minutes may be taken as an index of the capacity for muscular work at the particular rhythm. The results of each experiment are recorded in Table III, and in the last column the average amount of work done per minute is shewn. It will be seen that it falls off fairly steadily the slower the rhythm, till it is only 68 per cent. as great when 15 pulls were made a minute instead of 40.

TABLE III.—*Capacity for Work at Different Rhythms.*

Rhythm.	Strength of pull in 1st min.	*Mean.*	Strength of pull in 5th to 10th mins.	*Mean*	Work done per min. (5–10 mins)
Contr. ½ sec : relax 1 sec	204, 196, 191, 197, 206 ..	199	100, 99, 109, 118, 104 ..	106	106 × 40 = 4240 (100)
Contr. ½ sec : relax. 1½ secs.	239, 181, 195, 213, 210 ..	208	119, 113, 107, 140, 140 ..	124	124 × 30 = 3720 (88)
Contr. ½ sec. relax 2 secs	220, 209, 203, 200, 213, 203	208	135, 158, 136, 131, 145, 133	140	140 × 24 = 3360 (79)
Contr. ½ sec. relax. 2½ secs.	187, 210, 209, 208.. ..	204	153, 167, 178, 168 . ..	167	167 × 20 = 3340 (79)
Contr. ½ sec. relax. 3½ secs.	222, 224, 223, 226.. ..	224	194, 193, 200, 177.. ..	191	191 × 15 = 2865 (68)

THE INFLUENCE OF REST PAUSES ON A SUBJECT SHEWING A MARKED EFFECT.

The two subjects of experiment differed considerably in their response to the introduction of rest pauses, C.G.W. being much less influenced than I was. In two series of experiments on myself the duration of the rests was in the proportion of 1 of rest to 4 of work, and in a third series, with a faster rhythm, in the proportion of 2 of rest to 3 of work. During the rest periods I stood over the dynamometer, as in the work periods, but in half of the experiments I remained in an absolutely unchanged posture, whilst in the other half I kept on altering my posture by straightening my back, or bending slightly backwards, whilst at the same time I held the dynamometer handle alternately in one hand or the other, and gently moved the free arm about, and more especially twisted the forearm from side to side. I never did any actual massage, but the mild changes of posture mentioned had a most extraordinary effect on my recovery from the fatigue of the preceding bout of work. Proof of this statement is shewn in the parallel pairs of records shewn in Figs. 7 and 8, and 9 and 10. In each pair the upper record shews the effect of maintaining an unchanged posture during the rests, whilst the lower record shews the effect of change of posture. In Figs. 7 and 8 are seen the effects of alternately working for 8 seconds and resting for

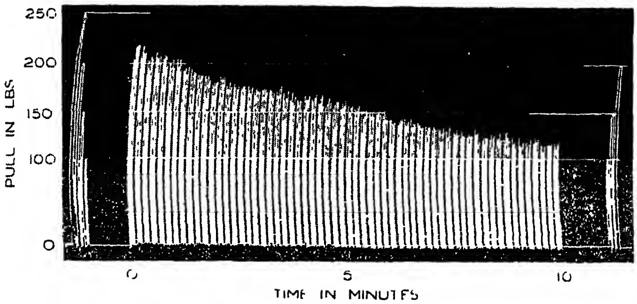

Fig 7 Work 8 secs : rest 2 secs Posture unchanged
(C/R = $\frac{1}{2}$/1$\frac{1}{2}$ secs)

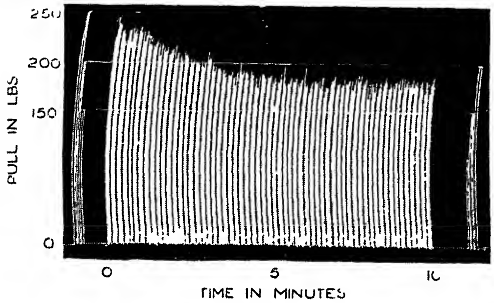

Fig. 8 Work 8 secs . rest 2 secs. Posture changed
(C/R = $\frac{1}{2}$/1$\frac{1}{2}$ secs)

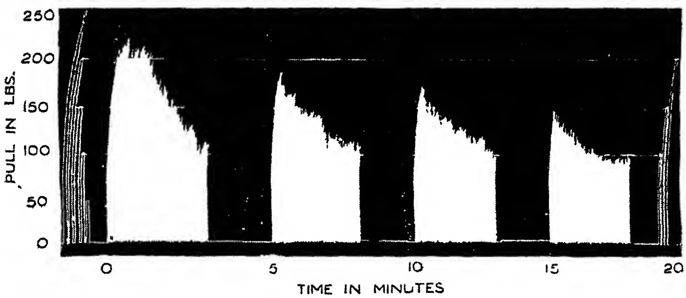

Fig 9 Work 3 min : rest 2 min Posture unchanged.
(C/R = $\frac{1}{2}$/1 sec)

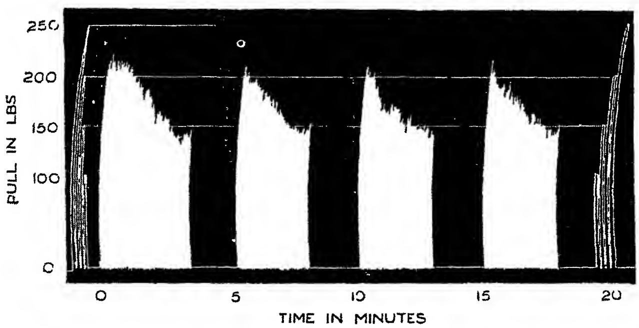

Fig 10. Work 3 min : rest 2 min Posture changed.
(C/R = 1/1 ...)

2 seconds, the handle being at the optimum height and the rhythm C/R = ½/1½ seconds. In Figs. 9 and 10 the work period lasted 3 minutes and the rest period 2 minutes, but the relatively longer rest was practically neutralised by the faster rhythm employed (C/R = ½/1 second). The records reproduced are average samples of the series, but in order to see the degree of consistency attained at different times it is best to compare the plotted results from the curves. In the next two Figures are

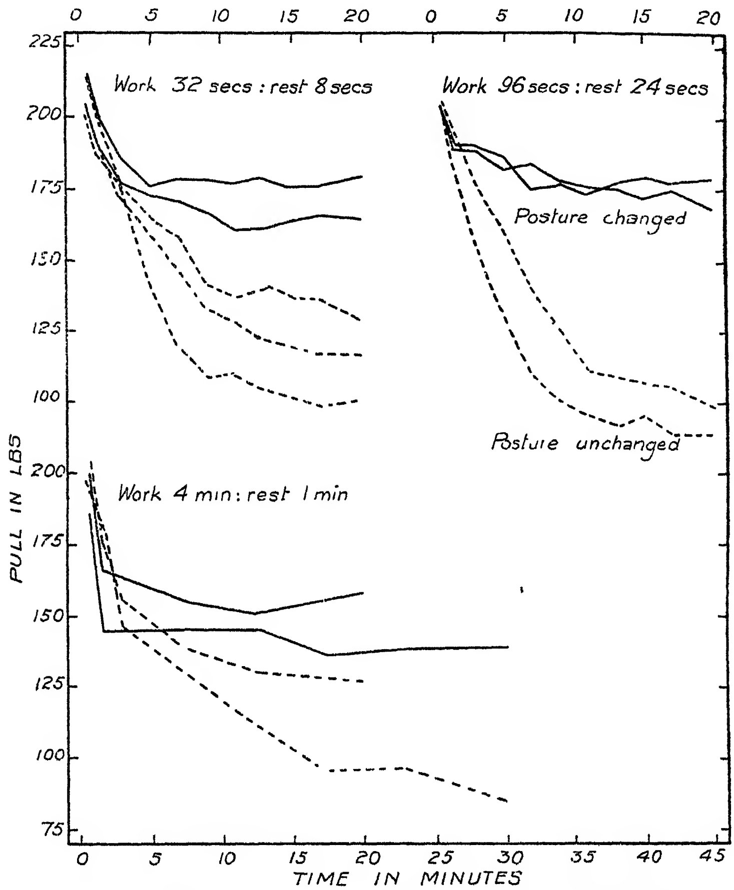

Fig. 11. The effect of various durations of rest pause (handle at optimum height: C/R = ½/1½ secs.).

plotted the experiments lasting 20 minutes or more, whilst numerical details of the 10-min. experiments are recorded in Tables IV to VI.

In Fig. 11 is shewn the effect of (*a*) working 32 seconds and resting 8 seconds ; (*b*) working 96 seconds and resting 24 seconds ; (*c*) working 4 minutes and resting 1 minute. The handle of the dynamometer was at the optimum height, and the rhythm was ½/1½ seconds. The "posture unchanged" experiments are

recorded as broken lines, and the "posture changed" ones, as continuous lines. The difference between the two series is most marked in (*b*), and least in (*c*), but it is very distinct in every case. In the posture-changed experiments there is little if any diminution in the strength of pull after the first 5 minutes, whilst in the others it falls off steadily for more than 10 minutes, and the "steady state" is barely attained in the last few minutes of the twenty.

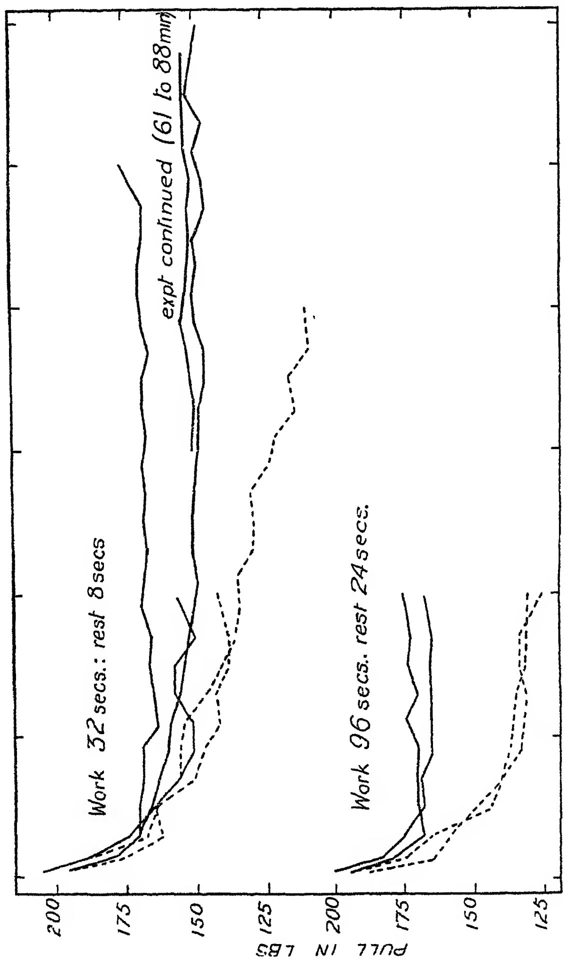

Fig. 12. The effect of various durations of rest pause (handle at straight back height: C/R = $\frac{1}{2}/1\frac{1}{2}$ secs.).

The corresponding series of results obtained with the $\frac{1}{2}/1$ second rhythm were so similar that it is unnecessary to reproduce them, but the mean pulls exerted in the 11th to 20th minutes are recorded in Table V. In the "straight back" experiments the effect of posture was less marked than in the others, owing to the less degree of fatigue produced, but it was quite distinct, as can be judged from the results plotted in Fig. 12. The system of working for 32 seconds and resting

for 8 seconds was found to be rather less tiring than that of working 96 seconds and resting 24 seconds, so most of the experiments of long duration were purposely made under these favourable conditions. One posture-changed experiment was continued for 50 minutes, and another for 88 minutes. In both of them the strength of pull kept almost level after the first few minutes, though it was 18 lb. less in the one experiment than in the other. At the time of making these experiments I did not feel particularly tired, and I could have kept on a good deal longer, but I felt rather tired in the evening and the next day, especially in the forearm muscles. I always made these long experiments on a Saturday afternoon, so that I had the Sunday in which to recuperate. Any fatigue effect on subsequent experiments was thereby avoided.

In order to determine what is really the most advantageous system of rest pauses, it is necessary to treat the results statistically. This has been done in Tables IV, V, and VI. The

TABLE IV.—*The Effect of Rest Pauses (Handle at optimum height : ½/1½-sec. rhythm).*

	Work and Rest Periods.	H.M.V. Pull in 1st min.	H.M.V. Pull in 5th to 10th mins.	H.M.V. Work per min.	C.G.W. Pull in 1st min.	C.G.W. Pull in 5th to 10th mins.	C.G.W. Work per min.
Posture unchanged.	No rests	208	119, 113, 107, 140, 140 =124	3720 (100)	232	189, 187, 193, 207, 205 =196	5880 (100)
	Work 2 secs.: rest ½ sec.	208	135, 158, 136, 131, 145, 133 =140	3360 (90)	235	207, 213, 198, 206, 210, 210 =207	4968 (84)
	,, 8 ,, ,, 2 ,,	214	129, 138, 132, 137 =134	3216 (86)	234	197, 202, 218, 200 =204	4896 (83)
	,, 32 ,, ,, 8 ,,	204	116, 123, 146, 155 =135	3240 (87)	221	203, 182, 212, 219 =204	4896 (83)
	,, 96 ,, ,, 24 ,,	202	122, 139, 141, 113 =129	3096 (83)	226	217, 195, 227, 221 =215	5160 (88)
	,,240 ,, ,, 60 ,,	200	125, 125, 129, 138 =129	3096 (83)	—	—	—
Posture changed.	Work 8 secs.: rest 2 secs.	215	180, 171, 179, 182 =178	4272 (115)	233	207, 204, 207, 219 =209	5016 (85)
	,, 32 ,, ,, 8 ,,	209	168, 170, 177, 180 =174	4176 (112)	218	187, 194, 199, 219 =200	4800 (82)
	,, 96 ,, ,, 24 ,,	208	171, 179, 180, 181 =178	4272 (115)	221	203, 211, 207, 209 =208	4992 (85)
	,,240 ,, ,, 60 ,,	195	136, 145, 155, 173 =152	3648 (98)	—	—	—

TABLE V.—*The Effect of Rest Pauses on H.M.V. (Handle at optimum height : ½/1-sec. rhythm).*

	Work and Rest Periods.	Pull in 1st min.	Pull in 5th to 10th mins	Pull in 11th to 20th mins.	Work per min. (5–10 mins.)
Posture unchanged.	No rests	199	100, 99, 109, 118, 104 .. =106	—	4240 (100)
	Work 1½ secs rest 1 sec.	208	135, 158, 136, 131, 145, 133 =140	—	3360 (79)
	,, 6 ,, ,, 4 ,,	206	124, 117, 134, 153.. .. =132	—	3168 (75)
	,, 24 ,, ,, 16 ,,	200	126, 148, 122, 135.. .. =133	112, 117 .. =114	3192 (75)
	,, 72 ,, ,, 48 ,,	199	114, 126, 134, 143, 124 .. =128	88, 101 .. = 94	3072 (72)
	,, 180 ,, ,, 120 ,,	209	135, 128, 138, 143 .. =136	113, 118, 133.. =121	3264 (77)
Posture changed.	Work 6 secs.. rest 4 secs.	211	174, 169, 175, 175.. .. =173	—	4152 (98)
	,, 24 ,, ,, 16 ,,	206	173, 177, 186, 184.. .. =180	185, 178 .. =182	4320 (102)
	,, 72 ,, ,, 48 ,,	202	185, 176, 167, 180.. .. =177	169, 178 .. =173	4248 (100)
	,, 180 ,, ,, 120 ,,	211	145, 171, 169, 165.. .. =163	167, 158 .. =163	3912 (92)

TABLE VI.—*The Effect of Rest Pauses (Handle at "straight back" height: ½/1½-sec. rhythm).*

	Work and Rest Periods.	H.M.V. Pull in 1st min.	H.M.V. Pull in 5th to 10th mins.	H.M.V. Work per min.	C.G.W. Pull in 1st min.	C.G.W. Pull in 5th to 10th mins.	C.G.W. Work per min.
Posture unchanged.	No rests	199	119, 133, 115, 142, 138 =129	3870 (100)	216	179, 178, 191, 167, 208 =185	5550 (100)
	Work 2 secs rest ½ sec	199	157, 159, 154, 164, 165, 153 =159	3816 (99)	208	192, 184, 187, 184, 205, 205 =193	4632 (83)
	" 8 " " 2 "	200	160, 155, 167, 148 =157	3768 (97)	208	165, 201, 197, 203 =192	4608 (83)
	" 32 " " 8 "	204	155, 147, 160, 151 =153	3672 (95)	208	192, 179, 203, 202 =194	4656 (84)
	" 96 " " 24 "	193	129, 150, 140, 142 =140	3360 (87)	202	189, 161, 187, 184 =180	4320 (78)
Posture changed.	Work 8 secs rest 2 secs	200	162, 163, 163, 164 =163	3912 (101)	233	207, 204, 207, 219 =209	5016 (90)
	" 32 " " 8 "	202	175, 170, 158, 169, 164 =167	4008 (104)	198	177, 170, 193, 194 =184	4416 (80)
	" 96 " " 24 "	189	151, 157, 171, 168 =162	3868 (100)	212	199, 187, 183, 167, 204 =188	4512 (81)

average strength of pull in the 5th to 10th minutes is recorded, not only for the experiments with the rest pause intervals just mentioned, but for two other schemes as well. In one of them I worked 8 or 6 seconds, and rested 2 or 4 seconds, according as the rhythm employed was ½/1½ seconds or ½/1 second. In the other, I took my rest pause after each pull. This rest amounted to ½ second and 1 second respectively with the two rhythms, so in actual practice I had to make only *one* set of (optimum height) experiments, with a rhythm of ½/2 seconds.

A consideration of the individual sets of data in these Tables shows that the frequency of the rest pauses had comparatively little influence on the mean strength of pull, and any small differences which actually exist are almost hidden by inevitable experimental variations. It is best, therefore, to average the results. This has been done by taking each set of mean values (for the 5 to 10-min. pull) as a unit, and turning the individual means into percentages. Then these percentages were averaged, and they yielded the following figures :—

Rest intervals every	2½ secs.	10 secs.	40 secs.	2 min.	5 min.
Relative pull of H.M.V...	105	101	101	98	95
Relative pull of C.G.W...	101	102	98	99	—
Relative pull of both ..	103	102	100	98	95
Relative pull of both (corrected)	103·2	102·6	102·3	105·0	118·7

It will be seen that the means of the results obtained by the two subjects shew a small but regular decline in strength of pull as the rest intervals were taken at less and less frequent intervals. Hence it might be concluded that the shorter the alternations of work and rest the better the result ; but this conclusion does

not hold if another factor be taken into consideration. In the experiments with rests at 2-min. intervals, for instance, the work done in the 5 to 10-min. period is finished, not at the end of 10 minutes, but 24 (or 48) seconds before that time, so it may be said that the total work of the 5 to 10-min. period is really finished in 336 (or 312) seconds instead of 360 seconds. That is to say, the relative amount of work done per minute comes to 105 (or 113) instead of 98. In the experiments with rests at 40-sec. intervals the work is completed only 8 (or 16) seconds before the end of the 10 minutes, so the work done per unit of time is really 102·3 (or 104·7) instead of 100. In the experiments with rests at 10-sec. and $2\frac{1}{2}$-sec. intervals the correction is correspondingly smaller, whilst in the 5-min. experiments it is correspondingly larger, but it must be remembered that in this latter instance it was possible to calculate only the work done from the 6th to 10th minutes (instead of the 5th to 10th), as there was no work done in the 5th minute.

Considering only the corrections applicable to the experiments in which one unit of rest was taken to four of work, we get the corrected values shewn in the bottom line of the Table. These values indicate that the strength of pull was practically the same when rests were taken at $2\frac{1}{2}$-sec., 10-sec., or 40-sec. intervals, whilst at 2-min. and 5-min. intervals it was considerably increased. The corrections would become smaller and smaller the longer the work was continued, and for a 5 to 20-min. period, for instance, they would be only a little more than a third as great as for the 5 to 10-min. period in question. If the experiments were continued still longer the results might be expected to approach more and more to the uncorrected values given in the Table. Hence we cannot say with certainty what is the effect of taking rests at different intervals, but in any case it appears to be small. In the experiments made by Maggiora (1890) with the Mosso ergograph it appeared that with comparatively long work periods the fatigue effects tend to increase at a relatively more rapid rate than with short ones.

The actual amount of work done per minute during the 5 to 10-min. period of each experiment is shewn in Tables IV to VI. Keeping as a standard the work done when no rests were taken, it will be seen that in my posture-unchanged experiments there was a distinct falling off. In the quick rhythm experiments this fall amounted to 24 per cent., and in the slow rhythm ones (optimum height) to 13 per cent., but it was only 5 per cent. in the straight-back experiments. When the posture was changed, however, the work done was slightly greater in the straight-back experiments, whilst in the optimum height experiments (slow rhythm) it was 12 to 15 per cent. greater. In the quick rhythm experiments the work done in the presence of rests was about the same as in their absence, but a more favourable result would have been obtained if the rests had been shorter, as was proved by a special series of experiments.

In this series, made with change of posture at the ½/1-sec. rhythm, the work period was kept throughout at 24 seconds, but the rest period was varied from 4 seconds to 24 seconds. The average results obtained are plotted out in Fig. 13, and it

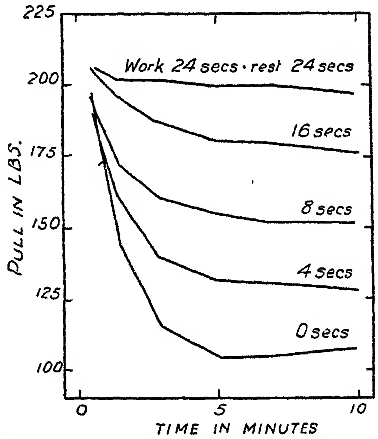

Fig. 13. The effect of various durations of rest pause, work period being constant.

will be seen that the contraction height varied approximately in proportion to the duration of the rests. Numerical details of the experiments are given in Table VII, and in the last column is recorded the amount of work done in an interval of 3½ minutes (this being the shortest time for which it is possible to calculate all the different systems of work and rest periods). It will be

TABLE VII.—*Capacity for Work with various Durations of Rest Period (½/1-sec. rhythm).*

Work and Rest Periods.	Pull in 1st min.	Pull in 5th to 10th mins.	Work done per 3½ mins.
No rests	199	100, 99, 109, 118, 104.. =106	140 × 106 = 14840 (100)
Work 24 secs.: rest 4 secs. .	193	124, 137 .. .. =130	120 × 130 = 15600 (105)
" " 8 " ..	197	153, 153 .. .. =153	105 × 153 = 16065 (108)
" " 16 " ..	206	173, 177, 186, 184 .. =180	84 × 180 = 15120 (102)
" " 24 " ..	206	196, 202 .. .. =199	70 × 199 = 13930 (94)

seen that this work reached its maximum when the work periods of 24 seconds were alternated with rest periods of 8 seconds, the total work done being 8 per cent. greater than when no rests at all were taken.

It is to be remembered that the results described afford no information about the oxygen consumption of the subject in the various series of experiments, or the degree of fatigue experienced. The actual mechanical work done in a given time is the only criterion available, and it is possible that if the oxygen consumption and fatigue had been estimated in addition somewhat different conclusions as to the most advantageous system of rest pauses might have been arrived at.

The Influence of Rest Pauses on a Subject shewing a Small Effect.

The data recorded on the right half of Tables IV and VI appear to show that change of posture had practically no influence on the work capacity of C.G.W. Arguing from the absence of the posture effect, it might be surmised that rest pauses would be of much less value to him than to me, and would under no circumstances enable him to do as much work as when he took no rests at all. This proved to be the case, for he always did from 10 to 22 per cent. less work when taking rests, and on an average did 16 per cent. less work in the optimum height experiments, and 17 per cent. less in the straight-back ones. That is to say, the rests improved his powers, when he was actually working, by about 4 per cent., for the rest pauses taken represented a loss of 20 per cent. in the total working time.

The extraordinary difference in most of the results yielded by C.G.W. and by myself is chiefly due to a fact which, when first noted, was thought to be of practically no significance. When pulling at the dynamometer, I kept my back almost immovable, and my shoulders moved backwards less than half an inch at the height of each pull. On the other hand, C.G.W. had got into the habit of bending his shoulders back at each pull, the average amount of movement amounting to about $2\frac{1}{2}$ inches. I thought it best that, having got into this habit, he should continue in it, so that his results might be uniform; but after the completion of the above series of experiments and of the subsequent series of "stand and sit" experiments described below, he made twelve fresh experiments in which he kept his shoulders steady during each pull as I had always done, whilst I made some in which I copied his method, and bent my shoulders back about $2\frac{1}{2}$ inches at each pull. In all of C.G.W.'s fresh experiments he worked for 96 seconds and rested for 24 seconds, the handle being at the optimum height. Six experiments were made at the $\frac{1}{2}/1\frac{1}{2}$-sec. rhythm, and six at the $\frac{1}{2}/1$-sec. rhythm. The results obtained are shewn in Fig 14. Here it will be seen that the posture-unchanged curves indicate a considerably more

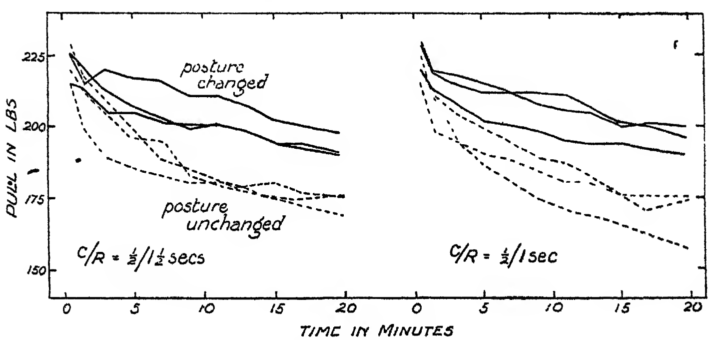

Fig. 14. Experiments by C.G.W. with shoulders not bent back during pull.

rapid fall of strength than the corresponding posture-changed curves. The average strength of pull in the 11th to 20th minutes shewed an improvement of 13 and 15 per cent. in the two series of experiments as the result of the postural changes, whilst in the corresponding series of experiments made three months before, in which the shoulders were bent back at each pull, the average pull in the posture-changed experiments was 4 per cent. *less* than in the posture-unchanged.

In the converse experiments made by myself, when I bent my shoulders back at each pull, there was still a distinct improvement of pull produced by postural changes during the rests, as can be gathered from the upper pair of curves in Fig 15. Each

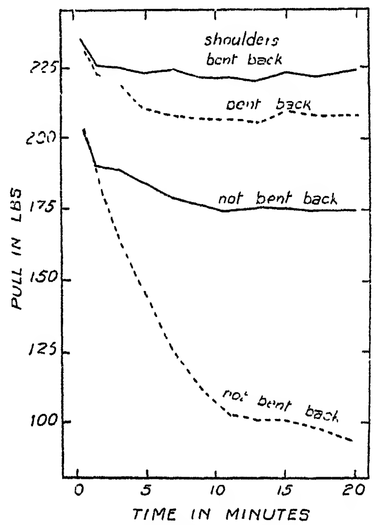

Fig. 15. Influence of bending back shoulders at each pull on the posture-change effect.

of these is the mean of three concordant experiments, whilst the lower pair of curves represent the mean results (obtained about three months before) of the corresponding experiments in which the shoulders were kept steady during each pull. (Work 96 seconds; rest 24 seconds; handle at optimum height; rhythm $\frac{1}{2}/1\frac{1}{2}$ seconds). The improvement of pull in these experiments averaged no less than 75 per cent., whilst in the fresh series under discussion it was only 7 per cent.

The Dependence of the Posture Effect on the Circulation.

The improvement in work capacity produced by changes of posture during the rest periods must be due largely to circulatory changes. The relaxation and change in position of the back and arms muscles must admit of a better flow of blood and lymph to the fatigued muscles, with a consequent acceleration in their rate of recovery. In fact, any movement of the muscles, followed by a relaxation, automatically improves the circulation through

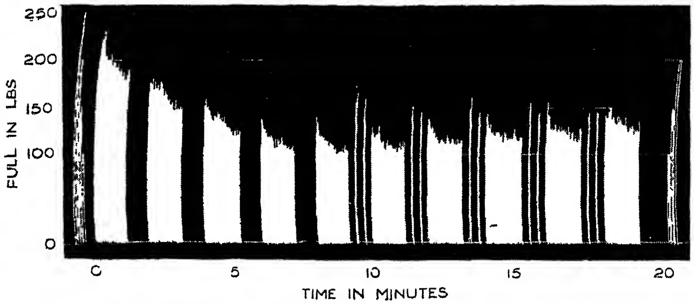

Fig. 16. Work 72 secs : rest 48 secs Posture unchanged, but in last half of experiment two pulls were made on two occasions in each rest pause (C/R = ½/1 sec.)

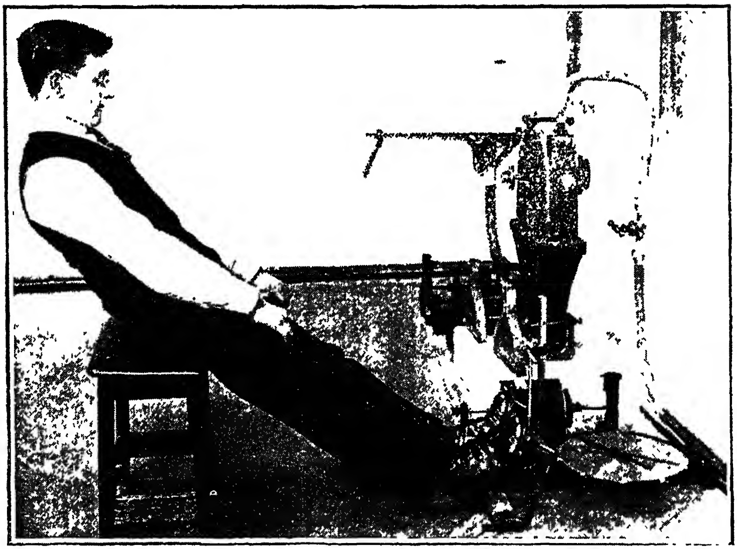

Fig. 18. Dynamometer being pulled in sitting position.

them, and thereby tends to diminish the fatigue effects, but as these movements of themselves produce more fatigue products, it is a question of discovering what movements can be made during the rests which cause the least production of extra fatigue products, but the greatest improvement of the circulation. C.G.W., by bending his shoulders back during each pull and forward during each relaxation, discovered one successful method of getting rid of fatigue products, and I myself happened to hit on another. I found that if, in a posture-unchanged experiment, the rest was broken by an occasional pull, the capacity for work during the subsequent work periods was considerably improved. For instance, in Fig. 16 is shewn the effect of alternating work periods of 72 seconds with rest periods of 48 seconds. For the first 10 minutes no movements whatever were made during the rests, and it will be seen that the strength of pull fell off rapidly; but in the last 10 minutes two pulls were made on each of two occasions during each rest, and the capacity for work continued to improve from that time onward. Other somewhat similar schemes were tried, and gave a like result. For instance, *one* extra pull was made every 5th, 8th or 10th second during each rest pause.

Another proof of the influence of the circulation depends on the after-effect of a hot bath. A bath, lasting 10 to 15 minutes, was taken 15 to 20 minutes before the experiment, and the results obtained are plotted out in Fig. 17. All the experiments

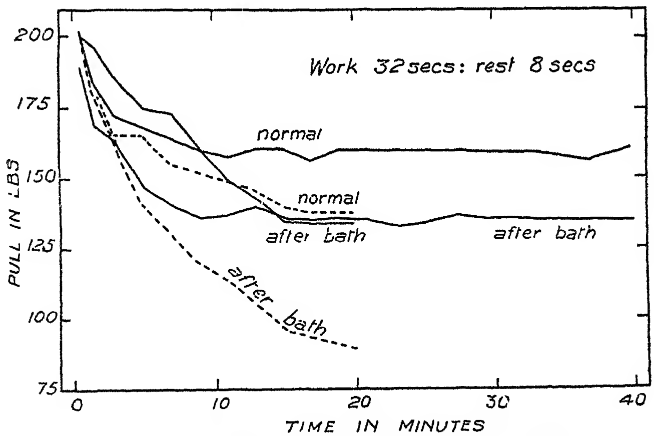

Fig. 17. Strength of pull 20 min. after a hot bath.

were of the straight-back type, with a work period of 32 seconds alternating with a rest of 8 seconds, and it will be seen that the two hot-bath experiments in which the posture was changed, and the one in which it was not changed, all shew considerably more fatigue than the mean results obtained under ordinary conditions, the curves of which are given for comparison. The hot bath presumably acts by diverting to the skin of the body some of the blood which would under ordinary circumstances be supplied to the fatigued muscles.

THE INFLUENCE OF ALTERNATELY STANDING AND SITTING.

A greater change of posture than that involved in bending the shoulders back during each pull, or moving the back and arms during each rest pause, was obtained by pulling the dynamometer alternately when in a standing and a sitting position. A strong iron foot-rest was fixed to the dynamometer in such a way as to offer no obstruction to the feet when in the standing position, and Fig. 18 shews the foot-rest in use. The stool, 17½ inches in height, was placed at such a distance from the dynamometer as was found to be most comfortable by each of us, and was always kept at the same spot. My distance was 33 inches from the wooden **T**-piece of the dynamometer, and that of C.G.W. was 26 inches.

The sit-and-stand experiments were begun seven weeks after the completion of the rest pause experiments above described, and were continued daily for a month. During their continuance the strength of pull improved in a most astonishing fashion, but the mean results obtained are closely comparable to one another, as great care was taken to mix up the order of the experiments so that no one kind had an advantage over another in respect of increasing strength of pull. It is to be borne in mind, however, that the experiments of this series are not directly comparable with those of the preceding series.

It was found that one could not conveniently change from the standing to the sitting position, or *vice-versa*, in less than 2½ seconds, so the following schemes of changes were determined on. Using the ½/1-sec. rhythm, we (*a*) made 25 pulls in 37½ seconds, and in the next 2½ seconds changed position and started pulling again. That is to say, we pulled alternately for 40 seconds when standing and 40 seconds when sitting. In scheme (*b*) we made 75 pulls in 112½ seconds, and had 7½ seconds to change position, and in (*c*) we made 187½ pulls in 281 seconds, and had 19 seconds to change position. That is to say, the relative amount of rest taken was the same in each scheme, but the changes of position were made every 40, 120, or 300 seconds. As a half pull was impossible in (*c*), 187 and 188 pulls were made alternately.

In addition to the sit-and-stand experiments, others were made in the sitting position only, and in the standing position only, with the same 2½ to 19 seconds of rest pauses. The sit-and-stand experiments were made twice, in reverse order, so that each complete test consisted of four experiments. These were made consecutively, though in varying order on different occasions. A sample set of tracings is shewn in Fig. 19, though for the sake of saving space one of the pair of sit-and-stand experiments is omitted. It will be seen that after the first 5 minutes I pulled a little more than 100 lb. each time when standing, and about 175 lb. when sitting. In the alternating experiment, however, I regularly pulled about 170 lb. when standing and 200 lb. when sitting. This tremendous increase in the strength of

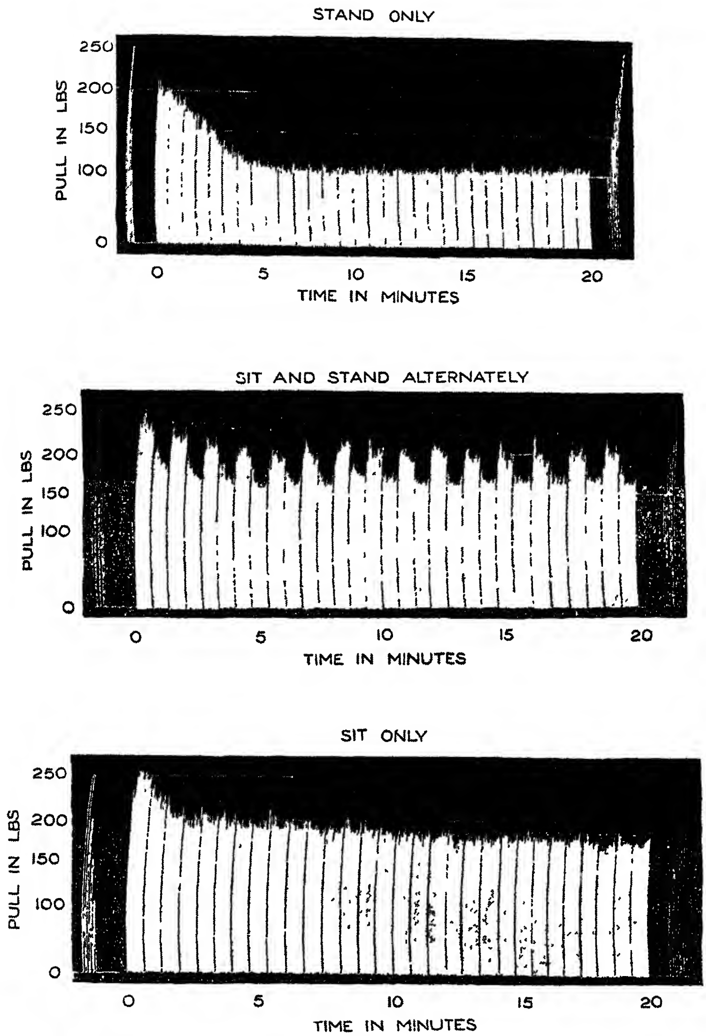

Fig 19 The effect of changing from the standing to the sitting position every 40 secs.

the standing pull produced by the alternations was borne out in the other experiments, and from the numerical data recorded in Table VIII, which are in each case the mean of three sets of experiments, it can be calculated that on an average my standing strength of pull was increased by 34 per cent., and my sitting strength by 4 per cent.; or the average stand-and-sit pull was 15 per cent. greater than the mean of the stand-only and sit-only pulls.

TABLE VIII.—*Strength of Pull with Alternations of Standing and Sitting.*

Work and Rest Periods.	Posture.	H.M.V.					C.G.W.				
		Pull in 1st min.		Pull in 11th to 20th mins.		Per cent. increase.	Pull in 1st min.		Pull in 11th to 20th mins.		Per cent. increase.
(1) Work 37½ secs.: rest 2½ secs.	Sit only	256	234	220	175		242	220	214	193	
	Stand only	212		131			198		173		
	Sit fraction	252	232	229	202	15	239	215	217	197	2
	Stand fraction	212		175			190		176		
(2) Work 112½ secs.: rest 7½ secs.	Sit only	243	226	209	172		218	203	189	178	
	Stand only	208		135			187		167		
	Sit fraction	244	225	218	194	13	226	207	211	192	8
	Stand fraction	206		170			187		174		
(3) Work 281 secs.: rest 19 secs.	Sit only	247	229	216	178		225	208	200	185	
	Stand only	210		140			192		169		
	Sit fraction ..	257	234	219	189	6	234	209	216	195	5
	Stand fraction	211		158			184		175		
(4) Work 38 secs.: rest 2 secs. for H.M.V. Work 112½ secs.: rest 7½ secs. for C.G.W.	Sit only ..	259	240	238	197		255	226	225	191	
	Stand only	220		156			196		158		
	Sit fraction	261	240	247	226	15	266	230	249	215	13
	Stand fraction	218		204			193		181		

The experiments with 2-min. alternations gave almost as striking a result, and on an average the stand-and-sit pull was 13 per cent. greater than the mean of the stand-only and sit-only pulls. The experiments with 5-min. alternations did not yield so striking a result, the average improvement produced by the alternations amounting only to 6 per cent.

The means of the 2-min. experiments are plotted out on the left side of Fig. 20. The stand-and-sit experiments are recorded as two individual fractions and as a mean of the fractions, whilst a mean of the stand-only and sit-only experiments is likewise shewn. The most noticeable feature is the tremendous improvement produced in the standing strength of pull by the alternations. This improvement must be due chiefly to the better circulation induced in the back muscles by the changes of posture. When pulling the dynamometer in the sitting position I used to throw back my shoulders several inches at each pull, and tighten up the body so that much of my body weight was brought to bear directly on the dynamometer, in addition to the force of my contracting muscles. For these reasons my average strength of pull in the sitting position was no less than 88 lb. greater than my standing pull during the 11 to 20-min. interval.

In addition to the three series of experiments mentioned, all

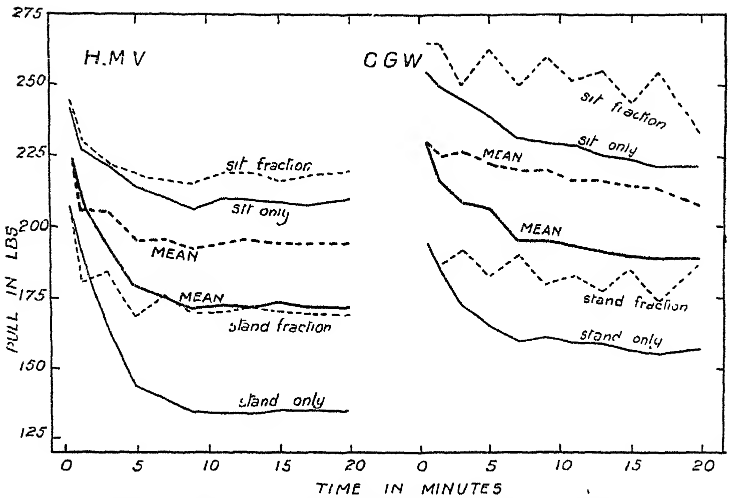

Fig. 20. The effect of alternately standing and sitting.

of which were made at the ½/1-sec. rhythm, I made a fourth series at the slower ½/1½-sec. rhythm, with 40-sec. alternations. The average improvement produced by the alternations was 15 per cent., as in the corresponding series of experiments at the quicker rhythm.

A repetition of the alternation experiments by C.G.W. gave a less striking result, and the numerical details recorded in Table VIII show that the average improvement in the first three series amounted only to 2 to 8 per cent. This smaller effect was due to the previously mentioned shoulder movements which C.G.W. made when standing. This is proved by the fact that when he carefully refrained from bending his shoulders back during each standing pull, he gave a similar result to mine. The means of three sets of experiments with alternations at 2-min. intervals are shewn on the right side of Fig. 20, and the numerical details, which are recorded in Table VIII (4), indicate a 13 per cent. improvement as the result of the alternations. The strength of pull is greater throughout than in series (1) (2) and (3), but this is merely because this series (4) was made by C.G.W. after he had completed the others.

The increase of strength during the course of the sit-and-stand experiments was very marked. As already mentioned, each set of experiments in each series was carried out three times. The second set was made about seven days after the first set, and my strength of pull (during the 5th to 10th minutes) improved 12 per cent. The third set was made about eight days after the second set, and my pull was now 29 per cent. greater than at first. This rate of improvement was about *eight times* more rapid than that experienced during the course of the preceding series of rest pause experiments, whilst the improvement shewn by C.G.W. was about five times greater. The cause of the enhanced

rate is thought to be as follows. In almost all of the rest pause experiments the strength of pull fell off considerably during the first few minutes owing to the accumulation of fatigue products in the muscles, and the average pull exerted by me in the 5 to 10-min. interval was under 150 lb. In the sit-and-stand experiments, on the other hand, and also in the sit-only experiments, the posture changes ensured a better circulation through the muscles (especially the back muscles), and there was much less accumulation of fatigue products with their consequent effect on the strength of pull. As a result, the pull exerted in the 5 to 10-min. interval was about 200 lb. in the first series of sitting experiments, and it increased to over 250 lb. in the third series. Owing to this considerably greater pull, the muscles were able to increase their power—presumably by hypertrophy—at a much greater rate than before. It appears to follow, therefore, that an accumulation of fatigue products, with its consequent lowering of the working capacity of the muscles, automatically reduces the rate at which the muscles hypertrophy as a result of functional activity.

Pulse Rate.—In the rest pause experiments, I counted my radial pulse for half a minute immediately before and after each 10 or 20 min. experiment. The individual results were rather irregular, as some experiments were done shortly after walking, and others after sitting quietly; but the average results were fairly steady as can be gathered from the data in Table IX.

TABLE IX.—*Effect of Work on Pulse Rate.*

Height of Handle above Floor Level		Posture unchanged.				Posture changed.			
		Pulse before	Pulse after.	In-crease	Stngth of pull	Pulse before.	Pulse after.	In-crease	Stngth of pull.
After 10 min. work.	Optimum height (slower rhythm)	92	112	20	133	84	106	22	171
	,, ,, (quicker rhythm)	89	111	22	133	87	112	25	173
	Straight back height (slower rhythm)	89	107	18	150	97	114	17	164
	Mean	90	110	20	139	89	110	21	169
After 20 min work	Optimum height (slower rhythm)	99	120	21	112	92	118	26	164
	,, ,, (quicker rhythm)	92	112	20	110	95	112	17	173
	Straight back height (slower rhythm)	89	109	20	138	100	113	13	164
	Mean	93	113	20	120	96	115	19	167

It will be seen that, as the result of 10 minutes' work, the pulse rate increased from 90 to 110 in the posture-unchanged experiments, and from 89 to 110 in the posture-changed experiments. This was in spite of the fact that 22 per cent. more work was done during the 5 to 10-min. interval in the latter series of experiments than in the former. Even more striking were the results of the 20-min. experiments, for 39 per cent. more work was

done in the 11 to 20-min. interval of the posture-changed experiments than in the others, but in spite of this the pulse rate shewed no more acceleration. It was practically the same as in the 10-min. experiments and, in fact, a mean of the two series shews exactly the same acceleration of pulse with the two postures.

The correspondence in the pulse acceleration does not warrant the assumption that the increased muscular work in the posture-changed experiments was performed with no greater expenditure of energy than in the others, as the quickening of the pulse depends on a number of causes. Sensations of discomfort and fatigue play a part, but we may at least conclude that changes of posture during rest periods enables the work to be done with greater ease, and with less strain upon the heart.

Practical Conclusions.

It might be thought that the results of laboratory experiments such as those described above can have but little direct application to industry, but this view seems to me erroneous, provided that conclusions are drawn with discretion. It will, no doubt, be at once admitted that the observations on the strength of pull at different heights above floor level are directly applicable to the subject of heavy weight lifting, and its liability to cause muscular strain ; but what conclusions can legitimately be drawn from the rest-pause and posture-change experiments ? There are at least two of great importance. Neither of them is new, but the experimental results serve to emphasise habits of industry which have long been recognised though frequently not acted upon.

The first conclusion is that when a rest pause is taken in the course of industrial work, it is important that the posture should be changed, even if the pause lasts only a minute. That is to say, operatives who have to stand at their work should sit down on as comfortable a seat as possible, whilst those who sit at their work should stand up, and still better, if it can be achieved without inconvenience, should walk about. Thereby the circulation through fatigued muscles is promoted, and fatigue diminished. A more important conclusion relates to changes of posture during the course of work. Such changes may be more efficacious than considerable rest pauses, even when systematic postural changes are made during the rests. Yet it is the exception, rather than the rule, for postural changes to be made during the course of industrial work. Many industrial processes can be carried on equally well in the standing and the sitting position, but in my experience (Vernon, 1924) the majority of workers, if they are provided with seats, remain glued to them the whole day long. Some of the more experienced ones get into the habit of standing up for a few minutes from time to time, but they are exceptional.

Another conclusion, the importance of which is problematical

though it may be considerable, concerns the effect of additional movements during muscular work. It can be deduced from the curves in Fig. 15 that if a subject, when pulling at the dynamometer, bent his shoulders back about 2½ in. at each pull, he could continue for 20 minutes with only 10 per cent. diminution in his strength of pull, but if he kept his shoulders steady his pull quickly sank to less than half its initial value. The slight shoulder movements improve the circulation through the fatigued muscles, and cause the more rapid removal of fatigue products. Now in many industrial processes of a repetition type it is the custom of some of the workers to sway their bodies backwards and forwards rhythmically in unison with the movements they are making with their limbs. In many cases these movements are undoubtedly unnecessary and undesirable, but before coming to a definite conclusion that they are all of this character, one should consider the possible or probable effects they might have in promoting the circulation through fatigued muscles.

One point very strongly brought out by the dynamometer experiments concerns their value as a fatigue test. We saw that in many or most of the experiments the strength of pull fell off very rapidly for the first few minutes, and then remained nearly steady for as long as the test was continued, even if it were 88 minutes; i.e. it had attained the "steady state." It by no means follows, however, that there was no increase of fatigue. Undoubtedly the general state of fatigue of the body continues to increase at a considerable and accelerating rate, in spite of the fact that the capacity for work, as tested by the dynamometer, remains constant. For instance, the sit-and-stand experiments proved particularly tiring to me, though there was often but little indication of fatigue in the records. When I first started this series of experiments I did three or four 20-min. experiments each day without feeling fatigued the next morning, but as my strength of pull increased I found the experiments more and more tiring, till at the end two a day were my limit. The last group of sit-only experiments, in which my pull fell off only 7 per cent. between the 1st and the 11th to 20th minutes, were particularly tiring, because of the tremendous pull I was exerting (over 250 lb. for 750 occasions in each experiment). Hence a test dependent on the estimation of the maximum strength of pull is no true indication of the condition of general fatigue.

Summary.

The strength of pull, or weight-lifting power, shews considerable variations at different heights above floor level. Taking the floor level value as 100, it gradually sinks to a minimum value of 72 to 81 at 14 to 17 in. above the floor, and then increases till it attains a maximum of 107 to 142 at a height of 28 in.

above the floor. At greater heights it falls rapidly. The weight-lifting power is only 2 per cent. less when the subject stands on one leg instead of two, but it is only just over half as great when he uses one hand instead of two.

When the dynamometer is pulled at regular intervals the initial strength of pull falls rapidly for about 4 minutes, and then for a long time keeps at a nearly constant level (the " steady state "). The height of this depends on the frequency of the rhythm, being 53 per cent. on the initial height when contractions were made every 1½ seconds, and 85 per cent. on it when made every 4 seconds.

In most of the experiments on rest pauses, the duration of the rest periods was to that of the work periods as 1 to 4, or as 2 to 3. If the subject of experiment remained motionless during the rest period, the total work done was always less (sometimes 28 per cent. less) than when no rests whatever were taken; but if the subject gently bent his shoulders back and moved his arms about, these changes of posture caused a tremendous reduction in the fatigue effect. The total work done was 2 to 14 per cent. greater than when no rests were taken, and the strength of pull remained at a constant level for as long as the experiment was continued (e.g. 88 minutes). When no changes of posture were made during the rests, the strength of pull continued to fall for most if not all of the experiment.

The posture effect is probably due in chief part to the influence of the postural changes on the circulation, as was suggested by the following experiments. (1) It was found that if the strength of pull was determined (*a*) when standing; (*b*) when sitting; and (*c*) when alternately standing and sitting, the average strength of pull in (*c*) was 6 to 15 per cent. greater than the average of (*a*) and (*b*). Presumably the alternate standing and sitting movements, being of a somewhat different character, promoted the circulation. (2) If an occasional pull were made during the rest pauses of a "posture-unchanged" experiment, the fatigue effect was considerably diminished in spite of the extra work done, as each pull of itself promotes the circulation. (3) If the shoulders were bent back about 2½ inches during each individual pull, instead of being kept immovable, the fatigue effect was greatly diminished, and the strength of pull was almost as great when no postural changes were made during the rest pauses as when they were made. (4) If a hot bath were taken 20 minutes before an experiment the fatigue effect was very much more pronounced, presumably because some of the usual blood supply was diverted from the muscles to the skin.

Rest pauses of the same relative duration to the work periods were taken at intervals of 2½ seconds, 10 seconds, 40 seconds, 2 minutes, and 5 minutes. It was found that the capacity for work is very little affected by the length of the interval.

The pulse shewed no greater acceleration in the experiments when the posture was changed during rest pauses than when it was not changed, in spite of the fact that 30 per cent. more work was being done.

In conclusion, I wish to express my indebtedness to Mr. C. G. Warner for acting as a subject of experiment, and to him and to Mr. T. Bedford for assistance in working out the results. Also I am greatly indebted to Professor Sir Charles Sherrington for permission to carry out the investigation in the Physiological Laboratory, Oxford.

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PREFACE.

In the preface to a recent Report,* the Board have described how the increasing substitution of mechanical for manual processes is modifying the nature of the demands made on the worker, through the gradual disappearance of heavy manual operations and the retention only of manual operations such as involve light and dexterous work, and how in consequence of this change in methods of manufacture, the mental effects of work in industry are tending to become relatively more important than the bodily effects. For this reason the Board have thought it desirable to devote part of their resources to the study of repetitive work, involving little physical effort but some dexterity, and have already been able in two of their most recent Reports† to point to evidence indicating the beneficial effects of short rest-pauses and slight changes of activity introduced within the spell of work for operations of this type.

It appeared also to the Board that some knowledge of the more fundamental principles underlying the performance of light repetitive work might be obtained by an intensive study under controlled conditions of a suitable process, comparable to some extent with others actually carried on in industry. They accordingly approved the initiation of the present investigation, which is concerned with the performance of four girls engaged in stitching on canvas over two periods of five weeks (after the practice effect had been eliminated), the first on a time-rate and the second on a piece-rate remuneration

The limitation of the experiment to four workers prevents of course any wide generalisation, but the consistency with which some effects emerge throughout the investigation appears to justify the putting forward of certain tentative conclusions. There are, for instance, clear indications that the substitution of piece-rate for time-rate may bring about an increase in output. Other effects which seem to persist in work of this type are the individual differences in the subjects selected, and in particular the fact that the most "intelligent" subjects are not necessarily the best workers (though there are indications that very low "intelligence" is not compatible with proficiency); the existence of a characteristic output curve; and the influence of aggregation or working in groups, as shown by the similarity in the course of the output curves for the different workers.

It should perhaps be noted that, apart from the nature of the work itself, the other conditions of the experiment could not be

*Bedale, E. M and Vernon, H. M. (1924): The effects of posture and rest in muscular work.—*I.F.R.B. Report No. 29.*

† Vernon, H. M., Bedford, T., and Wyatt, S. (1924): Two studies on rest-pauses in industry.—*I.F.R.B. Report No. 25.*

Vernon, H. M., and Wyatt, S. (1924): On the extent and effects of variety in repetitive work.—*I.F.R.B. Report No. 26.*

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made entirely comparable with factory conditions. The daily hours, for instance, were limited to two spells of three hours each, and work was carried on on only four days in the week. It may be for this reason that the beneficial effects on production of rest-pauses interpolated within the spells, to which previous investigations of the Board have pointed, have in this experiment been found to be absent.

July, 1925.

AN EXPERIMENTAL INVESTIGATION INTO REPETITIVE WORK.

By ISABEL BURNETT, M.A.

CONTENTS.

I. REPETITIVE WORK AND MONOTONY.

The word monotony connotes an unpleasurable reaction to repetition. Various theories have at different times been put forward as to its nature and origin. The Health of Munition Workers Committee have defined it simply as "analogous to . . . a fatigue process in unrecognised nerve centres." A more specific theory is that of C. S. Myers* who suggests that monotony is due to the fatigue arising from the inhibitory effort necessary to maintain any special attitude such as must be maintained even when a frequently repeated action has become automatic. Experiments in America and Germany have tended to confirm this theory. Ranschburg† showed experimentally that repeated stimuli of the same kind caused the individual's reaction to become increasingly difficult, while stimuli of different kinds had no such effect. Münsterberg‡ proceeded further along these lines and discovered that the difficulty in reacting to repeated stimuli varied with different individuals. He also quoted instances of factory workers who had expressly stated that they preferred repetition work. These two facts he thought were connected and suggested that the workers who preferred repetition work were the individuals for whom, owing to some innate psychophysical structure, a series of similar reactions to repeated similar stimuli was easy. Münsterberg defined monotony as "subjective dislike to uniformity or lack of change in work," the dislike arising from the individual's difficulty in making the same reaction a great number of times.

More recently, further research has been carried out by Winckler.§ This tends to show that the "subjective dislike" which represented monotony to Münsterberg is actually a feeling of fatigue arising from the continued effort to re-inforce stimuli which through repetition have become relatively ineffective.

The special feeling, more suitably called ennui or boredom, which according to the above definitions induces monotony, must not be confused with ordinary neuro-muscular fatigue. The latter, though it may often accompany monotony, is not an essential element in it. Thorndike||, as quoted by Muscio,¶ indeed, has shown that we can feel fatigued without any objective

* *Lectures on Industrial Administration.* Edited by B. Muscio. London 1920.

† Über die Bedeutung der Ähnlichkeit beim Erlernen, Behalten und bei der Reproduktion. *Zeit. f. Psych. u. Neurol.* **5.** 1905.

‡ *Psychology and Industrial Efficiency.* Leipzig 1914.

§ *Zeit. f. angewandte Psych.* **20.** 46–87.

|| Feeling Tone in Industry. *Brit. J. Psych.* **12,** 150.

¶ *Psychol. Rev* 1900. **7,** 481.

indication of the presence of fatigue. A very complete fatigue can be experienced after a prolonged trial on the ergograph, and in this case is induced by repetition when "monotony" may be absent. On the other hand, fatigue of the kind to which Myers refers may be induced by repetitive mental work when natural impulses to react to distractions, to let the mind wander, or any other mental process incompatible with the performance of the work must be inhibited or voluntarily controlled. For the effort required for inhibition itself uses up psychic energy. Hence the feeling of fatigue is induced, not by the work, which may not be at all fatiguing, but by this voluntary process of inhibiting other impulses. For this reason a change of occupation rather than rest is often more effective in counteracting this so-called fatigue or ennui, since by a change of occupation some of the natural impulses may be released, the effort of voluntary inhibition is dispensed with, and a normal mental equilibrium is restored.

In the experiment which is to be described there was evidence, even among so few subjects, of characteristic individual differences in the reaction to a repetitive task. For two subjects the work appeared to be more trying than for the others. This difference did not show itself in quantity of output but was gauged partly from their variability in reaction as measured by the fluctuation in their output curves, and partly by their behaviour during the fourteen weeks while they were under constant observation, and by their own remarks and comments at the end of that period.

II.—EXPERIMENTAL WORK.

The aim of this experiment* was three-fold :—

(i) to observe the different effects of repetitive work on workers of varying intelligence ; †

(ii) to observe the effects upon operatives and output of various rest-pauses ;

(iii) to compare the effects of time-rate and piece-rate remuneration.

It was divided into two parts, separated by an interval of six months, the first being carried out on a time-rate, and the second on a piece-rate basis.

* Carried out in the Psychological Department, University of Manchester, and under the supervision of Professor T. H. Pear.

† In the opinion of some employment officers and welfare workers in this district, where there is much repetitive, or so-called monotonous work, it is the more intelligent workers who are affected by the amount of repetition in their work. They soon require a change in occupation. It is reported that a local firm does not care to employ highly intelligent operatives on this account. This opinion of the management is confirmed by the findings of this experiment.

Time Rate Remuneration.

The four subjects were chosen from a school for Unemployed Young Persons. The selection was based on intelligence tests including directions and adding tests. It was hoped that the last would also give some information concerning the temperamental make up of the subjects.

Of the four subjects chosen as occupying different positions in the scale of intelligence, two, " A " and " B," ranked as very intelligent, " C " had average intelligence, and " D " had less than average intelligence as measured by these tests.

The repetitive work given them was cross-stitching with a coarse silk thread on canvas, 20″ sq. with a wide mesh of ·3″. The diameter of the thread was ·05″. This work was decided upon after careful consideration because it was thought that :—

(i) initial skill would be about equal ;*

(ii) output could be measured exactly by counting the number of stitches ;

(iii) there would be no wide range of quality; either the stitch would be correctly or incorrectly done ;

(iv) it was hoped that eye-strain would be practically eliminated because of the size of the stitching.

Work of this type could never become automatic and required constant watching on the part of the subject. Nevertheless, only very slight mental effort was involved, and no high degree of attention was necessary to ensure accuracy. It should be mentioned that this work is comparable with many factory occupations.

Hours and Methods of Work.

The subjects worked for six hours a day, in two spells of three hours each, for four days a week. There were four series of experimental methods, each tried on successive days of the week for two months.

Series I. *Tuesday* (the first working day of the week).—The subjects were allowed to pause when they felt inclined to do so, *i.e.*, they could take the advantage of unorganised rest-pauses.

Series II. *Wednesday.*—A rest-pause of fifteen minutes was given in the middle of both the morning and afternoon spells.

Series III. *Thursday.*—The subjects worked at maximum speed (without rest-pauses) both morning and afternoon spells, and, moreover, they were not allowed to talk.

Series IV. *Friday.*—Three rest-pauses of five minutes each were distributed at even intervals during the morning and afternoon.

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* Subject D, however, was exceptional and made a very bad start.

Friday was the last working day of the week. The subjects were paid then (30*s.* a week each). The time-rate basis of remuneration was without doubt an important factor, and its importance was determined when this experiment was supplemented by another, similar, except for the substitution of piece-rate for time-rate.

Factors affecting the Attitude of the Subjects to their Work

The work room provided by the laboratory for this experiment represented, as far as was possible, actual factory conditions. The walls were bare, and the lower parts of the windows opaque. The four subjects sat round one large table, and the experimenter remained at another in a corner of the room. No check was placed on talking except on silence days, and it was, indeed, from the general conversation that introspective information about the subjects' individual attitudes was derived.

During the course of the experiment the following information appeared in the conversation of the subjects :—

(i) In their own opinion they had a " soft job."

(ii) They were receiving more money than most girls of their age (17 years) for fewer hours of work.

These facts are of considerable psychological importance. This temporary work was the girls' first job after several months' unemployment. As a precaution against any subject leaving before the experiment was ended, it was decided to pay rather more than the ordinary rate and to make conditions of work as favourable as possible. So the interests of the subjects were biassed in favour of their work, and this, together with the novelty of the situation, perhaps did much to do away with the peculiar strain of monotony which is often associated with repetitive work.

Method of Measuring Output.

The output could be measured exactly by counting the number of stitches worked, at stated intervals. The subjects worked on alternate pieces of canvas, and at a given signal dropped one piece and resumed the other. Thus measuring the output did not interfere with continuity of the work. On Tuesdays the output was measured every half hour. On Wednesdays it was measured at the ends of three equal periods before and after the rest-pause of $\frac{1}{4}$ hour. On Thursdays it was measured every half-hour. On Fridays, morning and afternoon were divided into eight equal periods excluding the three short breaks of five minutes during both working spells. Thus a rest-pause came at the end of every second period. Daily output curves were obtained for each subject, representing her output for each period, and from these, analytic and synthetic curves were derived. It was also possible for the subjects to have a general idea both of their own and one another's output.

Results.

Among the time-rate daily output records there occur very frequently certain curves with a similar shape. These curves show the initial spurt of most work curves, fall very low somewhere about the middle, after which they rise almost to the end, where again they fall very low.*

If the table (*v. infra*) is consulted it will be seen that these curves appeared almost exclusively on Tuesday and Thursday, were much more frequent in the afternoon than in the morning, and occurred very often in the cases of A and B, less often in the case of C, and never in that of D. Fig. 1, Graph III, which illustrates the shape, particularly in the cases of A and B, was obtained from the average periodic output of each subject for the last three Tuesday afternoons during the experiment, when presumably the effects of the novelty of the situation as well as the practice effects had worn off. It will be observed that, except for the first period, C's curve is also similar in shape. These curves have been very tentatively called "monotony" curves for these reasons; (a) similar curves have been previously observed in unpublished records of experiments on repetitive work; (b) they are most frequent on those days when no rest pauses were given, i.e., when one would most naturally judge that monotony might have been present; (c) they occur chiefly in the work records of the most intelligent subjects, both of whom seemed to experience boredom as a result of the work, A giving evidence of this fact by her behaviour during the experiment, and B explicitly stating it at the end. We refer again to the general opinion of factory staffs that the most intelligent workers do, in fact, find repetitive work very monotonous.

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* Individual curves of this type were obtained on the following days:—

1st afternoon ..	Tuesday ..	particularly B's output curve.		
3rd afternoon ..	Thursday ..	,,	A, B & C's ,,	curves.
6th afternoon ..	Wednesday ..	,,	A, B & C's ,,	,,
7th afternoon ..	Thursday ..	,,	A, B & C's ,,	,,
9th morning ..	Tuesday ..	,,	A & B's ,,	,,
9th afternoon ..	Tuesday ..	,,	A.	
10th morning ..	Wednesday ..	,,	A.	
11th morning ..	Thursday ..	,,	A.	
11th afternoon ..	Thursday ..	,,	A & C.	
13th afternoon ..	Tuesday ..	,,	A.	
15th morning ..	Thursday ..	,,	B.	
15th afternoon ..	Thursday ..	,,	A & C.	
19th afternoon ..	Thursday ..	,,	A, B & C.	
21st afternoon ..	Tuesday ..	,,	A & B.	
23rd afternoon ..	Thursday ..	,,	B.	
27th afternoon ..	Thursday ..	,,	A.	
29th afternoon ..	Tuesday ..	,,	A, B & C.	
30th afternoon ..	Wednesday ..	,,	A & B.	
31st afternoon ..	Thursday ..	,,	A, B & C.	

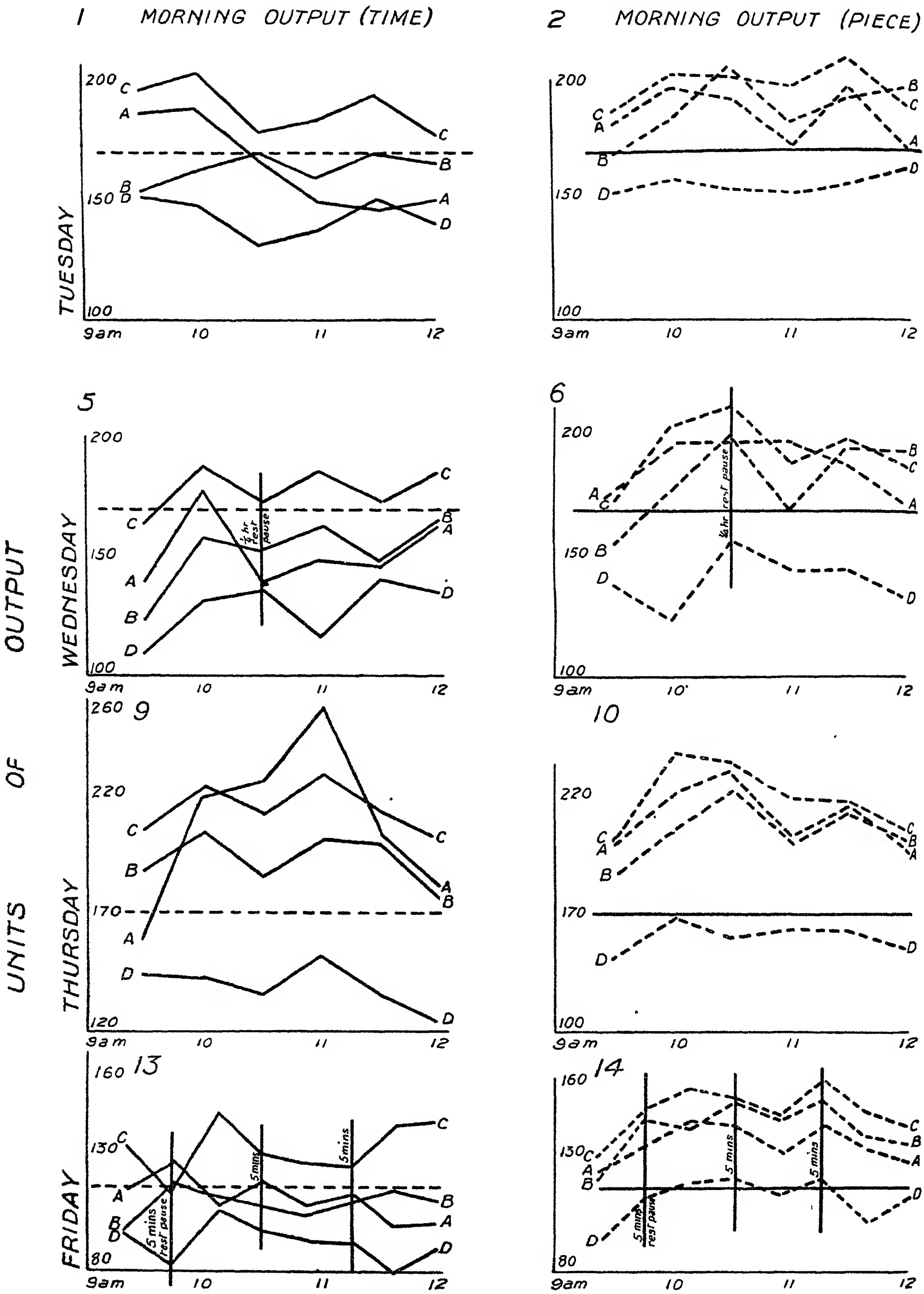
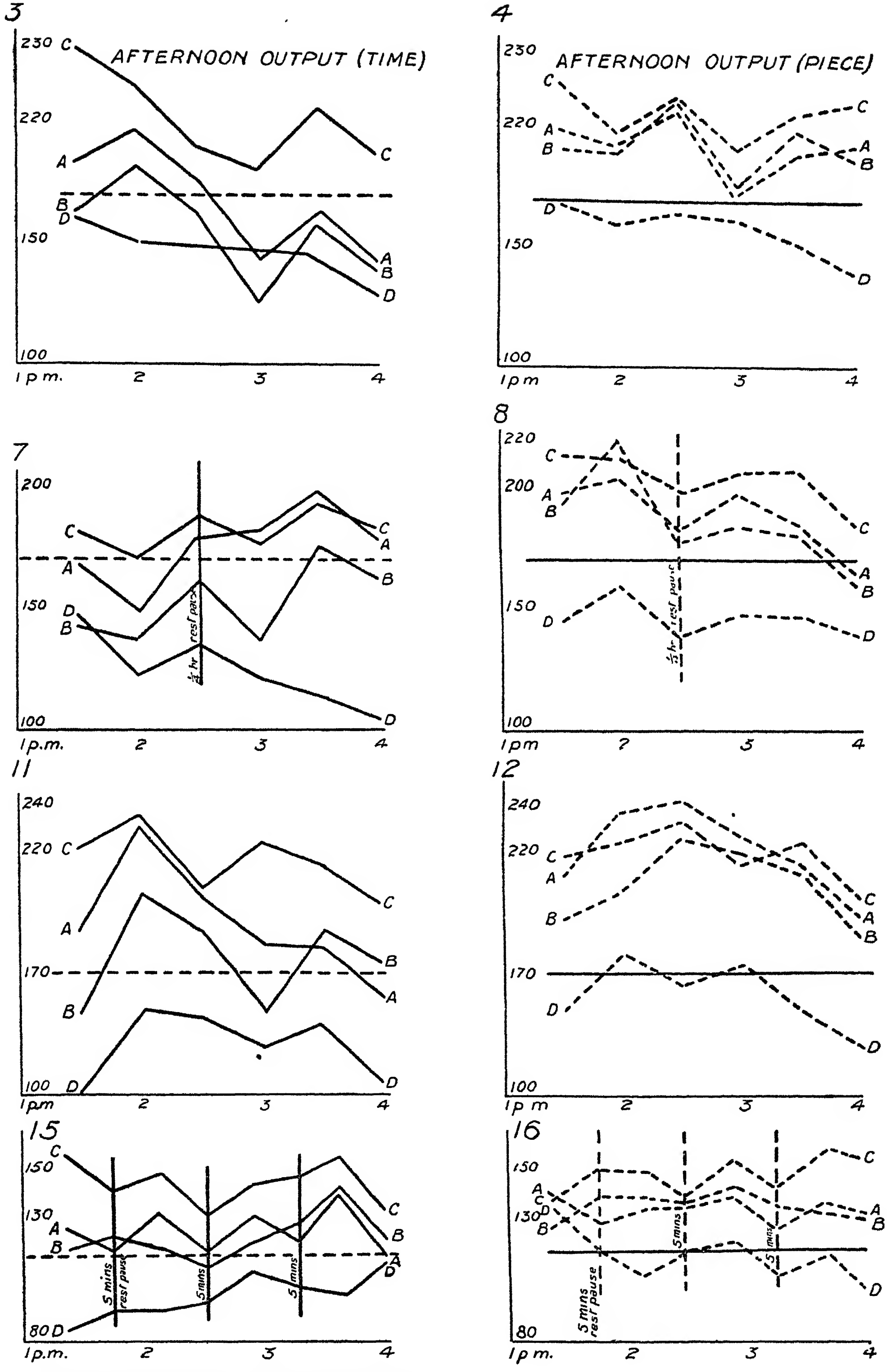


FIG. 1.—Average daily periodic output continuous lines obtained from time-rate experi-



curves for all subjects in both experiments:
ment; broken lines from piece-rate experiment.

Subject A. According to the tests (*see* Appendix), A was the most intelligent subject; her total output for the eight-week periods was second highest among the four subjects, but during the last three weeks it was only 88 per cent. of C's output, which was highest. A was capable of high single scores; in fact, she made the highest single score during the whole experiment (298 cross stitches in ½ hour). But she was not a steady worker. The highest percentage decrease from maximum efficiency is also hers, *i.e.*, 25 per cent.* Of the four, A alone showed physical signs of restlessness and boredom. She yawned, she frequently changed her position and sometimes stood up to her work; she welcomed any opportunity for variety, such as was provided by picking dropped needles from the floor, and she talked far more than the others, particularly of her hopes and plans for the future, how she would like to go to America, wanted a motor-car, and so on. Yet she was the second best worker in spite of her restlessness, and at the end of the experiment insisted that she had liked the work. (This may mean, however, that she liked the *conditions* of work.)

Subject B, who also ranked as very intelligent, was somewhat less efficient. Her output was 84 per cent. of C's and 4 per cent. less than A's output. Like A, she was capable of high single scores; but unlike A, she seldom made them. And she sometimes returned very low single scores. At the end of the experiment B confessed that she had found the work "very tedious" and would not like to do it regularly.

Subject C was ranked as third in respect of intelligence, but she was by far the best worker. She did 12 per cent. more work than A, 16 per cent. more than B, and 31 per cent. more than D. Moreover, her percentage decrease from her maximum output was smallest—14 per cent. against A's 25 per cent., B's 20 per cent., and D's 22 per cent. When asked at the end of the experiment, this remarkably steady worker said she had not experienced any strain of monotony as a result of repetitive work, but that, on the contrary, she had liked it.†

Subject D, whose intelligence according to the tests was less than normal, was also last in output. However, in spite of a very bad start, she made considerable progress. Her curve over the period of the whole experiment shows a remarkably steady practice effect (Fig. II). Though at the commencement she seemed hardly able to hold and control her needle and extremely clumsy in picking it up from the floor, where she very frequently dropped it, at the conclusion she was not far behind B in her

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* *Cf.* the erratic behaviour of A's curve derived from the adding test. (Appendix p. 25.)

† *Cf.* the smooth curve derived from subject C's "temperament" test. Here also her percentage decrease from maximum output is smallest. (Appendix p. 25.)

rate of output. During the eight weeks of the experiment she was not observed to volunteer any remark to the other subjects or to the experimenter, though she replied if addressed. At the end, when definitely asked for her opinion, she said that she had liked the work and had not found it monotonous, but that she would have preferred to work alone, as the talking of the other girls annoyed and disturbed her.

The introspective accounts of the subjects, their behaviour during the working spell, and their output curves in so far as they indicate the subjective attitude of each to her work, seem to show that of these four subjects the two most intelligent were least suited for this work. Though subjects A and B were capable of reaching a high output they could not maintain it. There was a conscious intermittent rivalry between the three good workers, and when this became acute, subjects A and B sometimes won the race; but on a dull day, when the monotony curve was in evidence, they dropped far below C. So their average output is considerably less, and their percentage decrease from maximum efficiency considerably more.

C was undoubtedly the best worker in all respects, although her intelligence score was much less than A's or B's. Her smooth output curve in the one-hour "addition" test (Appendix p. 25) indicate with remarkable accuracy her capacity for steady work over the longer period of eight weeks.

D's output during the last days of the investigation tended to be more erratic as it increased in quantity. Since she found conversation an unpleasant distraction and wished to carry on her work in solitude, it would seem that she had not suffered at all from monotony as a result of repetition work.

The Output under Varied Conditions of Work.

The other aim of this investigation was to find that spell of work most favourable for the operatives and the work. The time distribution for each day may be quoted again:—

Tuesday	..	Unorganised rest pauses.
Wednesday	..	Break of ¼ hour in the middle of morning and afternoon spells.
Thursday	..	No talking; no rest pause.
Friday	..	Three short breaks of five minutes in both morning and afternoon spells.

During the last three weeks of the experiment most work was done on Thursday when there was no rest-pause and no talking,

in spite of the fact that Thursday was much disliked by all subjects and evidently very fatiguing. Tuesday,* the day of unorganised rest pauses is next, when, however, only 90 per cent. as much work was done. Friday, with short frequent breaks is next, with 6 per cent. less work done than on Tuesday, and Wednesday's output is slightly less than Friday's.

These results cover only the last three weeks of the experiment, when effects of practice seemed to have ceased. For a seven weeks' period a different order obtains, with more work done on Friday than on Tuesday. However, on the six hours' working day which was observed in this investigation, most work was done without rest pauses, and least when a break of ¼ hour was introduced into the middle of the working spells. On Wednesday the subjects worked only 83⅓ per cent. of the time they worked on Thursday, but they did 84⅓ per cent. as much work as they did on Thursday. So there was evidently a slight speeding up in the rate of work, but not enough to make up for the rest pause.

Individually, subjects A, B and C did most work on Thursdays and subject D did most on Tuesdays. Subjects B, C, D did least work on Wednesdays, while A did least on Fridays.

No subject appeared to like the ¼-hour break on Wednesday; it had actually to be enforced on several occasions. It would certainly be too much to assume from this that all rest-pauses would have been unfavourable to the output of these subjects; but it seems clearly indicated that not all rest-pauses are favourable to output, and that generalisations are difficult to make about them.

Again, one must conclude that a relatively high output curve is not incompatible with what has been termed a "monotony curve." On Wednesdays and Fridays when rest-pauses were given there are only very rare indications of the "monotony" curve, but there is also a much lower output. The highest output occurs on Tuesday and Thursday afternoons, and the curves on these days have a similar and characteristic shape. Individual curves also show the same coincidence between high output and "monotony" curves.

Effect of Practice.

A complicating feature of the experiment was the effect upon output of practice, which extended in all cases over six weeks. For this reason most of the results of this time-rate investigation

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* Tuesday was the second best working day. This is more remarkable, since Tuesday was the first working day of the week, when usually there is a particularly low output.

have been drawn from the last three weeks only. The most remarkable and steady practice curve (v. Fig. 2) was obtained

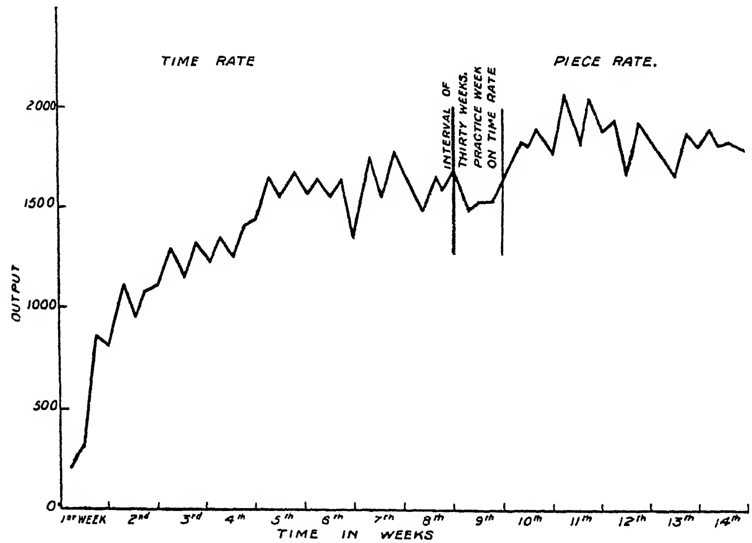


FIG 2.—Daily output of subject D throughout the investigation.

from subject D, who at the commencement was apparently hopelessly incompetent and unable to perform the simplest hand and eye and muscular co-ordinations (*e.g.*, she was quite unable to pick up her needle from the floor).

Co-operation and Rivalry in Work.

Not the least interesting feature of the experiment was the unspoken and probably unconscious rivalry between the subjects A, B and C, the unexpected effects of apparently unimportant stimuli, and the way in which the speeding up of one subject communicated itself to the others. The spontaneous outbursts of rivalry that occurred have already been mentioned. Fig. 3 (i) shows A and B starting a new piece of canvas simultaneously, which stimulated them even to surpassing C for the greater part of an afternoon. The same tendency is shown in Fig. 3 (ii), where the reason for the "race" is not known, whilst Fig. 4 illustrates the simultaneous drop in output due to a thunderstorm. On the other hand the subjects sometimes decreased their output of one accord for no apparent reason.

PIECE-RATE REMUNERATION.

The previous experiment was repeated six months later with the identical subjects and precisely similar conditions, except that the subjects were paid on a piece-rate basis instead of a time-rate as before.

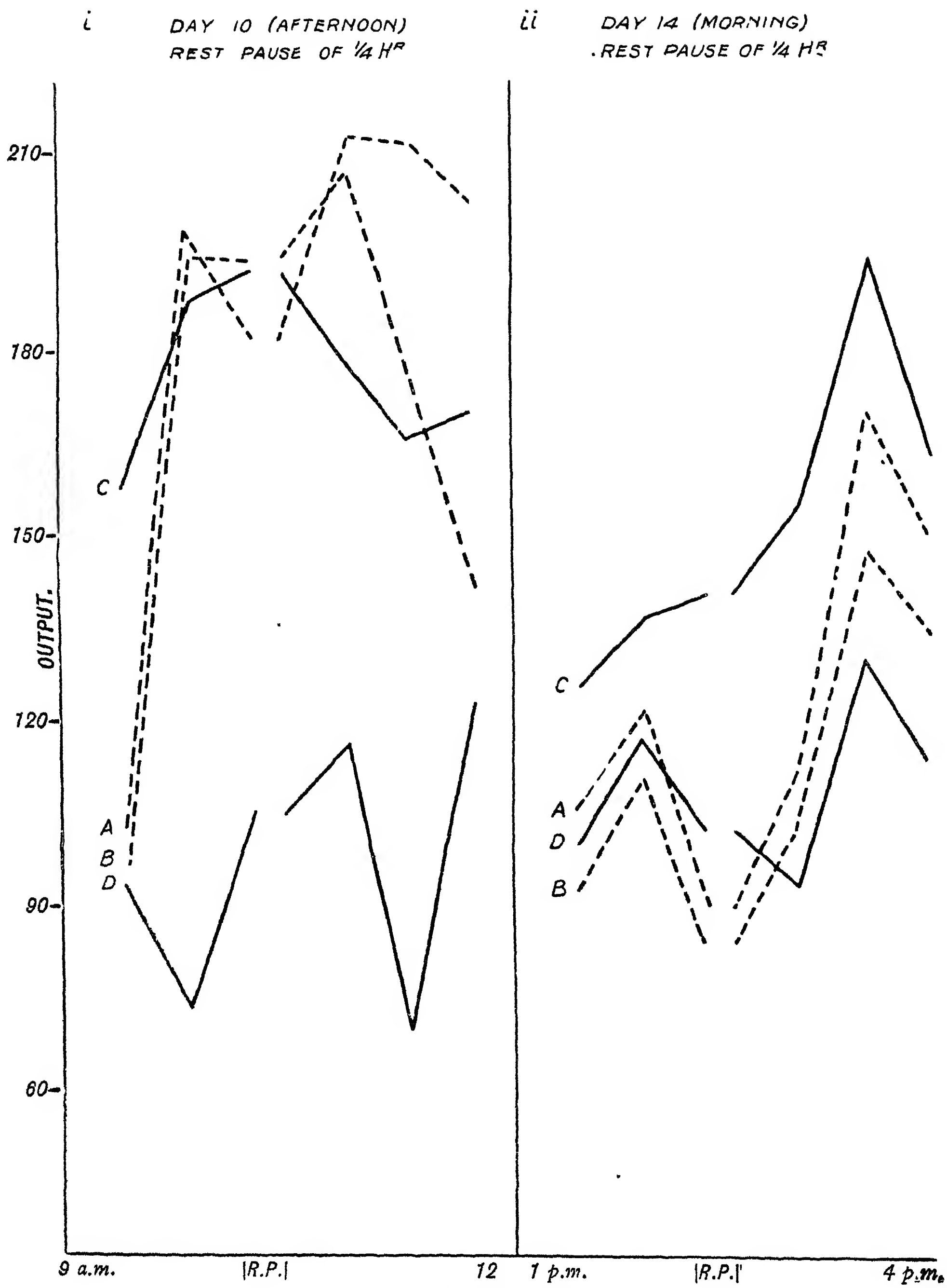


FIG. 3.—Effect of rivalry on output.

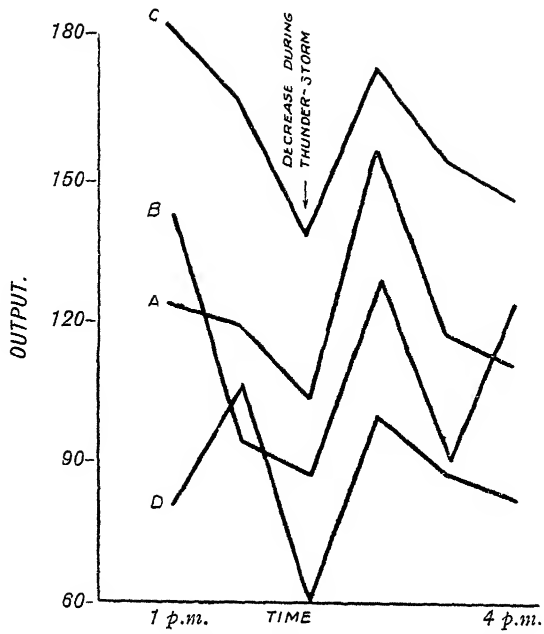


FIG 4 —Effect of thunderstorm on output.

The new piece-rate payment was struck from the average output of all subjects including D, who was a very poor worker. This method was chosen for D's especial encouragement, so that she should have the stimulus of a possible increase in her earnings. However, D's output was never high enough to bring her over the base line of payment, though her percentage increase was in the first week more than three times that of any other subject. This last fact indicates a successful rating from the point of view of the experiment.

Method.

As in the last experiment the subjects worked six hours a day in two periods of three hours, for four days a week. The method of each day was as before.

A preliminary working week on time-rate was thought advisable so that the effects of loss of practice in the interval between the experiments might be counteracted. However, it became clear that the subjects had suffered no loss of capacity in the interval, for the new working curve started with a higher score than that with which the previous curve ended.

General Results.

The immediate effect of adopting the piece-rate was a total average increase in output of 7·2 per cent. at the end of the first week, rising to 18 per cent. at the end of the second and

20·2 per cent. at the end of the third week. Following came a relative decrease to 10·8 per cent. increase on time-rate output at the end of the fourth week, and 7·9 per cent. at the end of the fifth.

It appeared that the early part of the curve which illustrated the weekly output of each subject (Fig. 1) was not complicated by practice effects, except perhaps in the case of D where the change to piece-rate seems to have prolonged the practice period. D ended with a higher output than that with which she began. This was not so in the case of the other workers.

The general results of a comparison between time- and piece-rate conditions can most readily be summed up by referring to Fig. 5, which illustrates the average periodic output of all subjects for each day both in conditions of time- and piece-rate payment. The points of comparison may be enumerated thus :—

(1) The piece-rate curves are on a much higher level throughout the day.

(2) While the time-rate morning curves show a slight tendency to drop at 10.30 a.m., the piece-rate curves have a very marked drop at 11 a.m. (slightly before 11 a.m. on Fridays). It is thought that the pronounced mid-morning drop on the piece-rate may be caused by two factors :—

(*a*) the subjects' relatively greater fatigue owing to the greater pressure that piece-rate imposes ;

(*b*) the subjects' need for food. (It is necessary to point out that all had a considerable journey before reaching the laboratory at 9 a.m. and hence were obliged to breakfast early.)

The incentive of piece-rate payment may account for the mid-morning drop being delayed for ½ hour (*cf.* time-rate curves) : the delay and the harder work account for the increase in the drop when it came.

(3) The tendency in the time-rate experiment to end the morning with a spurt is replaced in the piece-rate experiment by a tendency to drop. However, the curves do not cross. Even allowing for the drop at the close of the morning, the final points on the piece-rate curve are higher than those on time.

(4) Finally, whereas on time-rate the morning curves indicate no marked degree of fatigue and the afternoon curves are rather of that shape which has been called the " monotony curve," on piece-rate there are no definite monotony curves, but many more characteristics of fatigue curves are present. The difference is well illustrated by a comparison of Thursday afternoon curves under both conditions.

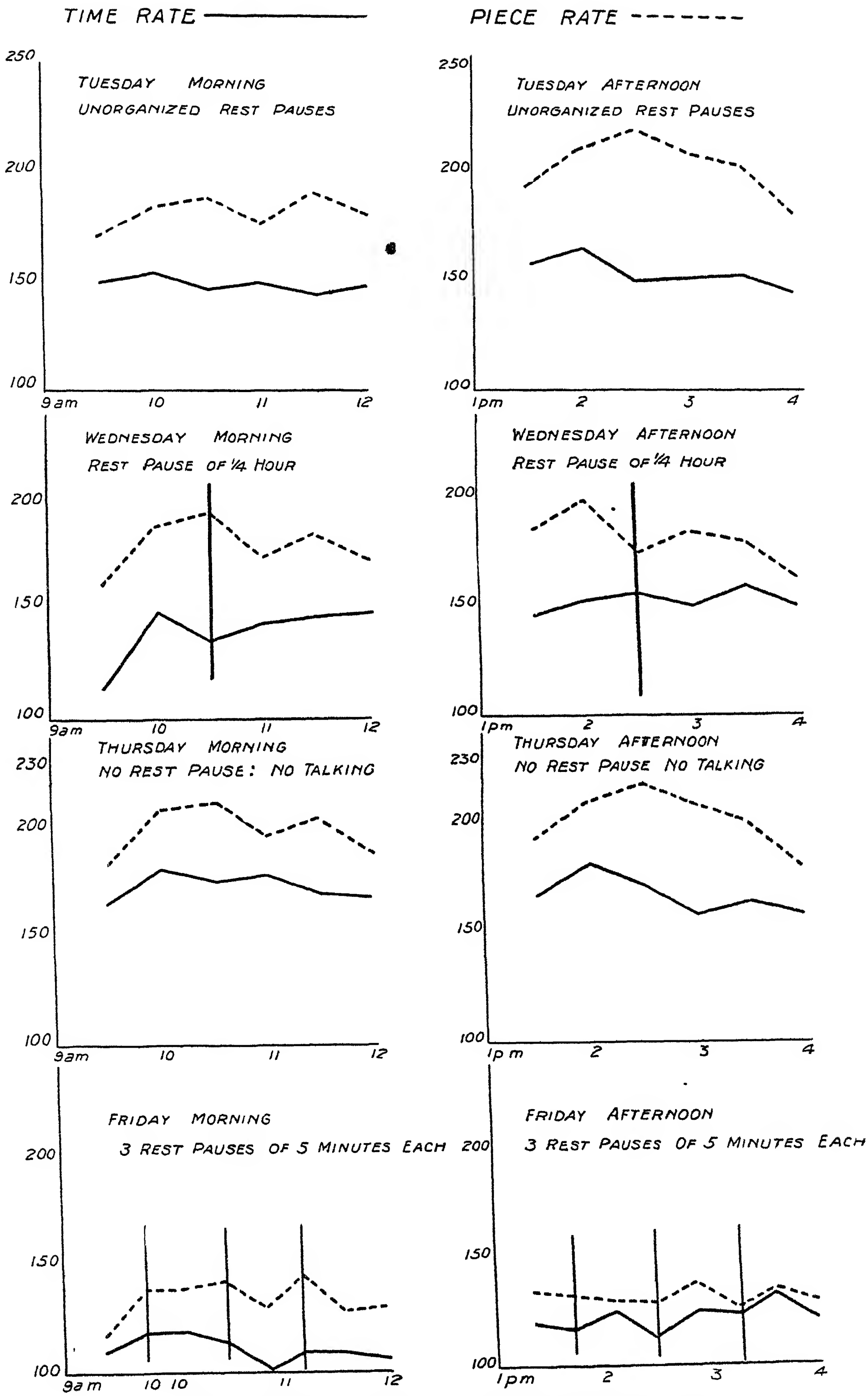


Fig. 5.—Average total periodic output curves for all days.

Comparison of the Average Periodic Output Curves of all Subjects on each day under Piece- and Time-Rates (*see* Fig. 1, p. 6).

Tuesday.—(Unorganised rest-pauses). Here also the effects of monotony are more clearly seen in the curves representing output on time-rate than in the curves representing piece-rate work. These curves are clearly seen in the graph of Tuesday (time-rate), in a lesser degree in the morning and very markedly in the afternoon. The curves illustrating piece-rate work are not dissimilar but are much less exaggerated. It will be noticed that the point of minimum output is nearer the end of the working spell and that the final spurt in two cases is greater than the initial spurt. The drop in the middle of the morning spell is only faintly indicated in the afternoon, probably because the afternoon working spell occurred at not so great a distance from a meal.

Tuesday afternoon piece-rate curves compared with Tuesday afternoon time-rate curves show the mid-spell drop interrupted by a rise in output. This Tuesday mid-afternoon spurt was probably not a conscious effort on the part of the three best workers: there is ample evidence that in this small group it was generally enough for one member to make a conscious effort to increase output for the others likewise to make a determined spurt. A most interesting and suggestive feature in this part of the experiment is the increasing closeness of the curves of subjects A, B, and C, especially *at the higher points.* Competition was a prominent feature in the time-rate experiment, as many of the daily curves showed, and it is a great aid to output as well as a means of alleviating monotony. In the piece-rate experiment spurts of rivalry were even more frequent. There is no doubt that this spirit of friendly competition acted as a mental stimulus to at least three of the subjects. (The case of D, however, is thought to be exceptional. D never reached the higher output scores of the other workers, though there is no doubt that she frequently exerted herself more than they did. After a while she seemed to become discouraged because of her repeated failures.)

Wednesday.—(Rest-pauses of $\frac{1}{4}$ hour in the middle of morning and afternoon spells.) The curve for Wednesday morning (time-rate) suggests that the rest-pause had been suitably placed, for it is followed by an immediate rise in three cases out of four and a delayed rise in the fourth case. There is also in three cases a considerable end-spurt. The corresponding curves for piece-rate, though on a higher level as a whole, are not satisfactory in shape. The rest-pause in this case has not done away with the mid-morning drop, which in three instances follows immediately after the rest-pause.

A comparison of time- and piece-rate curves for Wednesday afternoons is particularly instructive. Those illustrating time-work start low and in three cases end higher; the piece-rate

curves reverse the process, commencing very high then following with a considerable drop, which is arrested by a rest-pause. The rest-pause is followed in each case by a slight increase, after which output diminishes to the end. These piece-rate curves show plainly the effect of fatigue of the repetition work ; but at the same time it must be remembered that they are on a higher level than the time curves where no fatigue is indicated.

Now Wednesday, both in time- and piece-rate, was a day of relatively low output. Thus, on Wednesday (time-rate) the curves show a low output but no fatigue ; on Wednesday (piece-rate) we have again comparatively poor output with fatigue. Evidently with these subjects the ¼-hour break in the middle of the spell was not successful. In no case was the beneficial effect on the worker sufficient to make up for the loss of time. On Wednesday morning (piece-rate) in particular it seems to have been attended by bad results. But since in every instance the morning piece-rate curves show a minimum at or about 11 a.m., it is suggested that a somewhat shorter rest at 11 a.m. instead of ¼ hour at 10.30, particularly if some slight refreshment were taken, would be more beneficial both for the subjects and the results.

Thursday.—(No rest-pauses ; no talking allowed.) Thursday was disliked by the subjects (except perhaps D, who seemed indifferent), and even dreaded, or so it seemed, by A. The output was higher than on other days, but this was followed by a very poor output on Friday, whether as a result of reaction or of unsuccessful rest-pauses is not certain.

The morning curves (time-rate) do not show much fatigue until almost the end of the working spell. In three cases out of four there is the usual mid-morning drop at 10.30 from which the subjects recover without any rest-pause being given. The individual curves resemble each other in shape but are wide-spread.

The piece-rate morning curves slightly resemble monotony curves, but have this important difference, that the percentage decrease from maximum efficiency is considerably less than obtained in the time-rate. Throughout the piece-rate experiment it was noticeable that decrease from maximum efficiency, which has been taken to be one of the criteria of monotony, was much less marked than in the time-rate experiment. This factor is even more pronounced in the afternoon curves, where, at a glance, the range from maximum to minimum is seen to be much less.

The curves obtained for Thursday afternoon (time-rate) were typical so-called monotony curves ; those corresponding in piece-rate are clearly fatigue curves. Nevertheless, this does not seem to have been an excessive fatigue, for the final output scores are not greatly below the first scores. It is possible, however, that the true effects of Thursday's fatigue did not appear until Friday.

Friday.—(Three rest-pauses of five minutes, distributed at equal intervals throughout the morning spell, and three during the

afternoon.) It is true that with the insertion of three short rest-pauses during the morning and afternoon spells both fatigue and monotony seem overcome as far as can be judged from the curves, but the effect on output in all cases is bad. Often the rest-pause is followed by a drop in output, and the mid-morning drop still persists.

All the curves obtained from readings of output on Friday resemble each other in being widespread, *i.e.*, there is a large variation about the average points of all subjects at any given point. This was characteristic of all work done in time-rate conditions, but very considerably diminished with the introduction of piece-rate work, except in this instance of Fridays, where the variation among the subjects is still as great.

Friday's routine was liked by the subjects, but the output was very poor. To what extent the harsh conditions of Thursday affected the output of Friday is an interesting point which cannot be decided on the results of this experiment. It is, however, very possible that Friday's low output did to some extent represent a reaction against Thursday.

The working time unit on Friday was approximately twenty minutes against ½ hour on the other days, but in that period the subjects did not do as much as two-thirds of the work done in an average ½ hour on Thursdays, but rather less. Possibly this may be explained by assuming that the twenty minutes' working unit was too short, leading to too many interruptions, for on each of these occasions the work had to be changed, involving a certain cooling down and loss of swing.

Individual Differences in Time- and Piece-Rates.

*Subject A.**—Two curves, A's average periodic output curve for Tuesday morning and her average periodic output curve for Thursday afternoon, show fairly well her typical reaction to the conditions of the time-rate experiment. If the corresponding curves for the piece-rate experiment are superimposed the difference might be considered to indicate A's change in her attitude to the work—a difference that, it might fairly be assumed, was brought about by the change from time- to piece-rate. Figs. 6 and 7 show the curves obtained in the old and new circumstances. The Tuesday morning curves (Fig. 6) are in striking contrast. That obtained in time-rate conditions is a typical so-called "monotony" curve, having, moreover, only a very slight end spurt. The piece-rate curve, on the other hand, though it has a marked mid-morning drop, is much more buoyant to the last, finishing well above the time curve. Thursday afternoon curves (Fig. 7), however, resemble each other closely, the chief difference being that the piece-rate curve is on a higher level throughout, and in shape more markedly a fatigue curve.

* For description of the subjects' intellectual and temperamental characteristics v. p. 8 and Appendix.

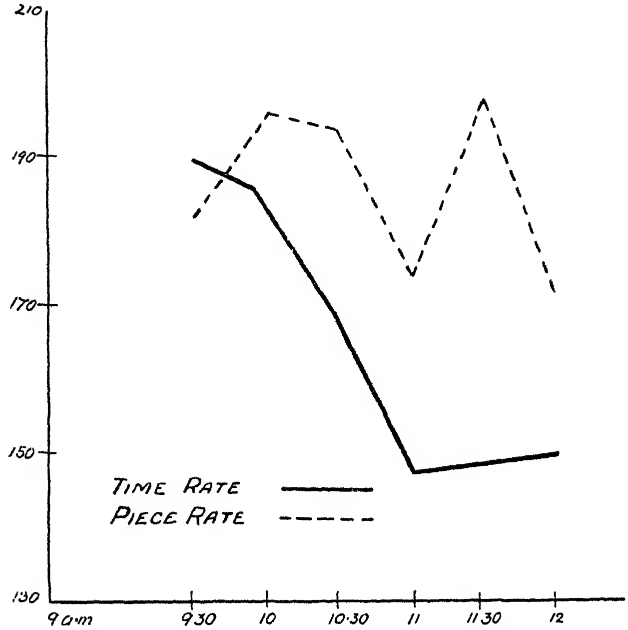


FIG. 6.—Average periodic Tuesday morning output curve (Subject A). (Continuous line=time rate; broken line=piece rate.)

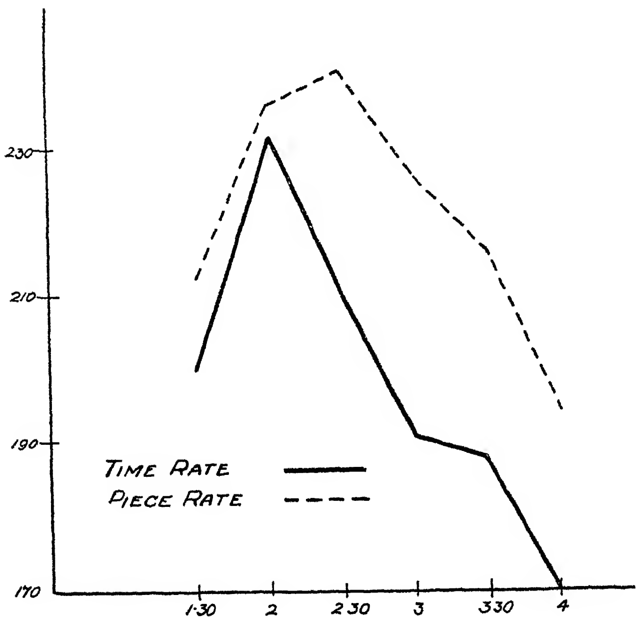


FIG. 7.—Average periodic Thursday afternoon output curve (subject A). (Continuous line=time rate; broken line=piece rate.)

A's average percentage increase over the five weeks of the experiment after going on to piece-rate is 13·9. The general air of restlessness and boredom, so marked in this subject in the previous experiment, was not nearly so marked in the piece-rate period. *Cf.* also A's curves for Tuesday afternoon, time- and piece-rate. (Fig. 1, Graphs 3 and 4.)

Subject B.—If B's curves for Tuesday afternoons on time and piece-rate be compared, the same difference is observable. (Fig. 1, Graphs 3 and 4.) The piece-rate conditions of work seem to induce not only a better output at the end of the day, but also yield a middle spurt. However, on Wednesday afternoon the results are less favourable to piece-rate—B finishing lower than when on time-rate.

On Thursday afternoon (time-rate) the curve which is thought to indicate monotony in B is changed into a more productive curve, which possibly also represents more fatigue. It is, however, on a higher level throughout.

B's average percentage increase after going on to piece-rate was 9·3 per cent.

Subject C.—Even in the time-rate experiment C showed fewer monotony curves and her variability was much less than A's or B's. C's Tuesday afternoon average periodic output curve (time-rate) is her clearest instance of a monotony curve; and the corresponding curve on piece-rate has not those characteristics, but has a middle spurt and an end spurt which bring C to a final point not much lower than the first point of the spell. As in the previous experiment, C's output was highest. Her percentage increase, however, was 10·1; or less than A's and D's, and only ·8 higher than B's.

Subject D.—The individual case of D is the most interesting. The change to piece-rate had an immediate and striking effect on her output; in the first week she increased her output by 16·1 per cent.—more than three times the increase of any other worker; and in the second week by 24·5 per cent. Unfortunately even this great improvement on D's previous work did not bring her above the base line of payment and she was still much slower than any other subject. Following came a relative decrease—probably due to discouragement—to 17·8 per cent. increase, dropping during the next week to 10·8 per cent., then, as if for a final spurt, rising again to an increase on time work of 16 per cent. Her average increase over the whole period was 17·9 per cent. against A's 13·9, B's 9·3, and C's 10·1.

Two factors may have contributed to D's failure to maintain her improved condition :—

(i) The general slacking off which was to be observed in all subjects after the third week;

(ii) the particular depression of D owing to her failure to rise above the base rate, though her increased efforts were proportionately much greater than the others.

On account of the latter reason it is thought that D suffered from her proximity to workers so much superior to herself, and from her inability to work as quickly and easily as they did. As in the previous experiment, D never voluntarily spoke to the other subjects and could not always be persuaded to answer when they spoke to her.

This seems to be of particular importance in reference to the many psychological problems raised by team-work in industry. That such team-work in certain circumstances may increase output has been strikingly shown. But it seems probable that an effective team should consist of members whose ability does not differ greatly. Otherwise, not only will the better worker be held back by the presence of the slower, but the discouragement of the slower worker by the mere presence of the faster may decrease her output, thus creating a vicious circle.

The Similarity of the Curves.

In both time and piece-rate experiments a remarkable feature has been the tendency of the individual curves to assume a similar shape throughout any working spell. This characteristic is particularly noticeable in the cases of A, B, and C, though not entirely absent in D's curves. If the curves of A, B, and C for the average periodic output of Tuesday afternoon on time-rate be compared with those on piece-rate (see Fig. I, p. 6), there will be found a dissimilarity in shape between the time and piece curves for which it is thought that the additional impetus of piece-rate payment will account, but there is this in common, that the curves composing each graph—whether for time or piece—tend to have the same shape amongst themselves. In the graph illustrating time-work, however, the curves lie wide apart: in the other case three curves lie closely together. This is not a unique instance—*cf.* also the graph of Wednesday afternoon (piece-rate); and Thursday morning and afternoon (piece-rate) and most of the other days in a less degree.

It is possible that this uniform rate of working (excluding D for the moment) is a characteristic feature of repetition work. When one worker consciously sets herself to speed up her work this speeding up tends to communicate itself to the other workers; and similarly the slacking of any one worker tends to take the form of restlessness and talking, which first disturbs, then infects the others. This working in harmony doubtless is a great aid to the alleviation of monotony. It seems also possible that this tendency, beneficial to the workers, should be utilised to the advantage of industry. In this experiment the workers sat very close together and except on certain days (Thursday in each week) there was no check to their conversation, which consequently often tended to be excessive and to the detriment of their work. This fact alone would account for many of the uniform drops

in the curves. It is thought, then, that the subjects should have been at a somewhat greater distance from each other so that conversations were less easy, while rivalry in output remained as easy as before. Talking is not necessarily and always undesirable, but it is a mistake to suppose that the benefits arising from working in a social group wholly depend on conversation. Conversation should neither be forbidden, nor yet encouraged by too close grouping. The workers should be within sight and hearing of each other, but not in small close groups. It is also instructive to notice that D, for instance, the poorest worker, seemed to feel her disadvantage more acutely because of the nearness of the others. It may be remembered that in the previous experiment she complained of being disturbed by their presence and wished that she could work alone.

Conclusions.

Summary of Time-rate Investigation.

It was found that of four subjects selected on a basis of individual intellectual and temperamental differences, the two most intelligent (on the showings of the intelligence test), and most erratic (on the showings of the adding test) were also the most variable workers on the findings of the eight weeks experimental test.

The subject with sub-normal intelligence, according to the intelligence test, showed on the other hand a remarkably steady improvement in her ability to perform the given work (v. Fig. 2 for D's practice curve). Subject C whose intelligence was average and whose output on an hour's intensive repetitive work showed a relatively smooth curve, was the best and steadiest worker in the more prolonged test.

Such data are suggestive. A comparison of school with factory careers might yield interesting and profitable results. On superficial evidence it has always seemed probable that the highly intelligent individual is not suited to repetitive work. The present investigation supports this opinion. A more extensive investigation might lead to the possibility of distinguishing this type before the unsuitable careers were commenced.*

Whether repetitive work can be said to induce the subjective experience of monotony seems to depend on the individual and on the circumstances of work. In this experiment the good payment, the novel situation, and the shortened hours probably were sufficient to overcome, at least during fourteen weeks, some part of that feeling of distaste which is usually thought characteristic of repetitive work. Nevertheless certain curves seem to

* This would necessarily be an exacting investigation, involving the accurate estimation of school children's intelligence and the comparison of these results with subsequent factory careers.

be characteristic of this sort of work and to indicate a particular kind of fatigue. These have been called "monotony curves." How far monotony affects efficiency depends entirely upon the individual. In this experiment, the monotony curves often coincide with high output curves.

As to rest-pauses, those employed in this experiment were unfavourable to output, though the short breaks which were given on Friday were much appreciated by the subjects. With an eight hours' day and a $5\frac{1}{2}$ days' week, however, the effect of the rest-pauses might have been different.

Summary of Piece-rate Investigation.

In an experimental comparison of time and piece-rate output in repetitive work, it has been found that with the piece-rate basis of payment :—

(*a*) the output curves are on a higher level throughout the day ;

(*b*) with workers of approximately equal capacity the variation about the average output is less than is found on time-rate ;

(*c*) amongst a group of approximately equally skilled workers, competition, which is not only a means of alleviating monotony but also an aid to output, occurs more frequently in piece than in time rate, and is closer ;

(*d*) but the output of a particularly unskilled worker is apt to suffer from the discouragement (or so it is thought) of repeated failures to make a score as high as that made by the other workers. If, for example, subject D had been working with a group of workers younger or less capable than herself, it is possible that she might have asserted herself in her work more successfully ;

(*e*) the effects of the change from time- to piece-rate were most marked in the most intelligent and most variable and least in the least intelligent and least variable of the workers.

APPENDIX.

Intelligence Testing at Higher Openshaw Central School for Unemployed Young Persons.

In order to ensure a variety of types even among the four subjects required for the experimental investigation of repetitive work, the following tests were applied to a group of thirty girls (average age 17 years) at the Higher Openshaw Central School for Unemployed Young Persons.

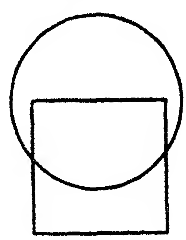
TEST I.

(*Compiled by Mr. J. A. Fraser, of the Manchester University Psychological Laboratory.*)

NAME OF SUBJECT ....

I.Q. ....

1. Put a stroke through the second "l" in the word "little."
2. Make two crosses under the shortest word in this sentence.
3. If the sum of 3 and 7 is less than 12 put the figure 6 between these two crosses X X.
4. If Z is the last letter in the alphabet, and if B does not come before A write No under this line.
5. Look at the line of figures at the end of this sentence and put a circle round the fourth figure to the left of 2, and a line under the second figure to the right of 5 :—7895831620974.
6. Look at the square and the circle. Put X in the space which is in the circle but not in the square, and Y in the space which is in the square and in the circle.



7. If the letters of the word NOR appear in the same order as they do in the alphabet, and if the same is true of the letters in the word AND, write the letter Z, but if this is true of only one of the words write the last letter of that word immediately after the last word in this sentence.
8. Look at the five rows of letters below and draw three lines which do not cross, connecting—

1. The	14th	and the	37th	letters.
2. do	2nd	do.	45th	do.
3. do	5th	do.	42nd	do.

B O X T U V A Z X.
H N G F T R E W Q.
A S D O T Q Y U L.
F B H N M T G K J.
A Z C H B M L K N.

This test applied to a group of school children gave a correlation coefficient of + ·60 (p.e. ± ·078) with Intelligence Quotients based on the Northumberland scale. Applied to a group of cotton operatives, it gave a correlation coefficient of + ·86 (p.e. ± ·036) with weaving efficiency.

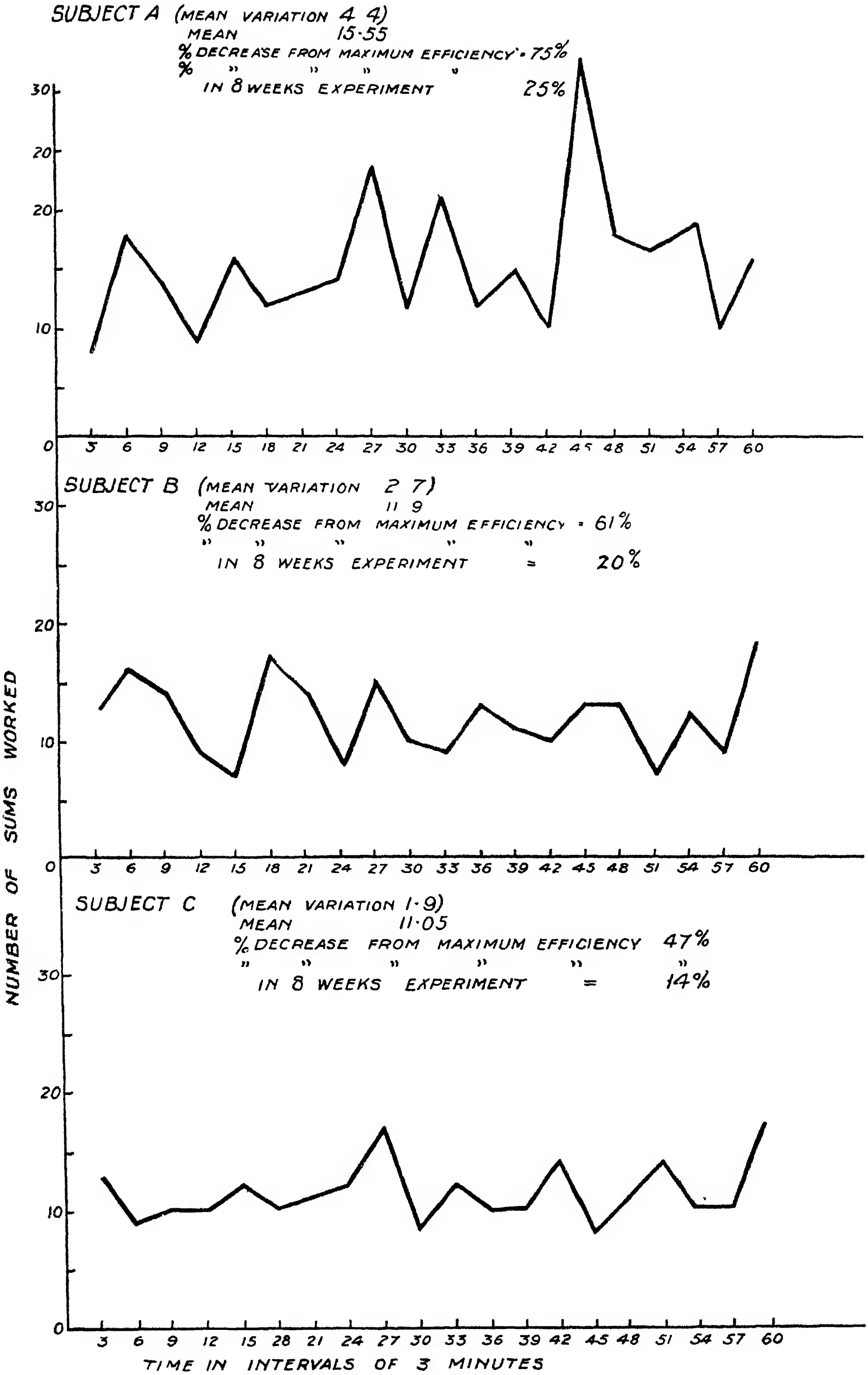

SUBJECT A (MEAN VARIATION 4 4)
MEAN 15·55
% DECREASE FROM MAXIMUM EFFICIENCY = 75%
% " " " "
IN 8 WEEKS EXPERIMENT 25%
SUBJECT B (MEAN VARIATION 2 7)
MEAN 11 9
% DECREASE FROM MAXIMUM EFFICIENCY = 61%
" " " " "
IN 8 WEEKS EXPERIMENT = 20%
SUBJECT C (MEAN VARIATION 1·9)
MEAN 11·05
% DECREASE FROM MAXIMUM EFFICIENCY 47%
" " " " " "
IN 8 WEEKS EXPERIMENT = 14%
NUMBER OF SUMS WORKED
TIME IN INTERVALS OF 3 MINUTES

FIG. 8.

TEST II.—ADDING TEST.

The classes added printed groups of figures (as below) for one hour, writing the answer beneath each column of digits and marking the number of sums completed every three minutes.

12	49	51	64	75	86	97	18	23	35	41	52	63	74	98	89
78	21	12	43	54	65	76	89	94	16	29	35	46	57	61	72
45	32	83	19	98	27	31	42	57	68	73	84	95	16	27	38
34	31	42	53	64	74	86	97	47	58	69	17	98	29	81	62
91	25	36	47	58	65	78	82	21	32	17	81	32	43	53	27
67	59	91	12	23	39	41	61	75	84	96	76	65	54	14	39

etc., etc.

The curves of three of the subjects afterwards chosen for the laboratory experiment are given in Fig. 8 (the fourth, subject D, did not follow instructions and consequently her rate of work could not be determined).

It will be seen that these curves, which illustrate one hour's intensive repetitive work, indicate very accurately some of the subsequent findings of the eight weeks investigation.

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INDUSTRIAL
FATIGUE RESEARCH BOARD.

Performance Tests of Intelligence

By Frances Gaw, B.A.

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REPORT No. 31.

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PREFACE.

As is stated in their Third Annual Report, the Board early in 1923 decided to collaborate with the National Institute of Industrial Psychology in carrying out a preliminary investigation on vocational guidance, consisting amongst other inquiries of first, the application of certain tests to children leaving school, and secondly, the determination of the extent to which the results of these tests show correlation with subsequent proficiency in the industrial occupations entered upon.*

In the course of the investigation, much use was made of "tests of intelligence," most of which have up to the present been designed on a linguistic or verbal basis, that is to say the answers to the questions submitted are expressed by means of words. It has been represented, first, that such tests, valuable as they are, may sometimes weigh unduly in favour of those who have a special facility for verbal expression, and may fail to discover intelligence, such as might be valuable for many industrial occupations, in those who have a special difficulty in using or understanding words, and, secondly, that since with all individuals it is desirable to measure intelligence by the most comprehensive means possible, a combination of linguistic and non-linguistic methods is probably preferable to the former alone.

In recent years, therefore, there has been a tendency to supplement the original verbal tests by others (designed chiefly by American psychologists) by which intelligence is measured, not by any oral or written response, but by the performance of some act. These "performance" tests of intelligence were originally devised for application either to mentally deficient persons to whom verbal expression was clearly a difficulty, or to foreigners ignorant of, or unfamiliar with, the language in which they were written. Of late years, however, they have been modified and applied, in conjunction with the other type of tests, to normal children entering industrial occupations.

It is, perhaps, still too early to appraise the full value of these tests; but in view of their probable importance in the future and of the fact that they are at present to be found described largely in scattered publications, the Board have decided to publish the

* The investigation is now being continued and extended by the National Institute of Industrial Psychology under a special grant from the Carnegie United Kingdom Trust, but the Board have thought it desirable to publish the results obtained up to the present stage, and a report on these is now under their consideration.

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present report (prepared by Miss Frances Gaw, who has had experience in the practical application of the tests in the United States), embodying descriptions of a number of those in common use. The tests described in this report have been found directly applicable to children in elementary schools under the London County Council. A partial restandardization of the tests has been accomplished with these children and certain minor changes made.

The Board are indebted to Messrs. Appleton & Co., Ltd. (London), Messrs. Sidgwick & Jackson (London), Messrs. Warwick & York (U.S.A.), The London County Council, and the Graduate School of Education, Harvard University, U.S.A., for permission to reproduce certain diagrams and extracts from books published by them.

April, 1925.

INDUSTRIAL FATIGUE RESEARCH BOARD.
REPORT No. 31.

PERFORMANCE TESTS OF INTELLIGENCE.

By Frances Gaw, B.A.

CONTENTS.

I. INTRODUCTION.

A.—Need for Performance Tests.

The present publication is concerned with the development and use of performance tests of intelligence. One series of such tests is here described in detail, and directions are given for its use. So many excellent tests of intelligence have appeared in the last two or three decades, that in presenting another series, or adapting previously used tests to make another series, a word of explanation is not out of place. The justification for the present scale of tests is that it offers a method of measuring intelligence by non-verbal means—a method which has received comparatively little attention from psychologists. The principal reason for the use of non-verbal methods is to get a more comprehensive view of mental ability than is afforded by linguistic tests alone, and to correct an over-emphasis on verbal methods of expression. In general it is fairer to judge an individual by both verbal and non-verbal tasks, rather than by verbal only, and certainly in specific cases, as, for example, in vocational guidance for children leaving elementary schools, it is of paramount importance to give both kinds of tests in order to decide whether an individual's abilities are manifested chiefly in a linguistic or a non-linguistic way.

Performance tests, because of their recent development, have not as yet been subjected to as much careful scrutiny and experimentation as have linguistic tests of intelligence. Nevertheless, scales of performance tests have been found not only satisfactory, but specially valuable for the range of years constituting roughly the elementary school period, namely from 6 to 14. Hence non-verbal tests of intelligence—and among them the series for which directions are given here—are available for three important uses; for vocational guidance, especially of children about 14, the age at which pupils leave the elementary schools; for mental defectives, and more particularly for dullards with mental ages of 8 and upwards; and for the large number of juvenile delinquents whose ages fall below 14.

B.—Historical Development of Performance Tests.

1. *Preliminary experiments and tests.*

Performance tests, so called, were first developed for measuring intelligence as an outcome of the successful researches of Itard and Séguin upon the method of educating mental defectives. Itard, a French physician, conducted experiments during the first half of the nineteenth century, notably with the "Wild Boy of Aveyron,"[1] and later with infant idiots. In training the "Wild Boy" Itard used various combinations of forms and colours. Among his devices was a square board upon which he pasted brightly coloured papers, a red circle, a blue triangle, and a black square. He gave the boy cardboard forms of the same shapes and colours which were to be matched with those on the boards. Other boards involving various forms and colours were also used. Séguin, also working in France with mental defectives, after numerous experiments published in 1866[2] a book containing his theories and his methods of training. He, like Itard, constructed a number of formboards and planned a series graduated in difficulty.

To this new and successful method of training defectives two developments may be traced. First, similar material has been used most advantageously by Montessori and other educationists in teaching normal children. Secondly, formboards of various sorts have been developed for measuring intelligence. These formboards, though they were not adapted solely and primarily for testing until some years later, furnished the material for the first performance tests.

During the last two or three decades many performance tests of different types have been developed in various countries. Not all these tests are formboards, and not all are primarily for measuring the intelligence of mental defectives, but whatever be their material and difficulties, they all have two outstanding characteristics in common. On the basis of these two resemblances, performance tests can be defined as short mental problems, which may be presented and must be solved in non-verbal terms. Thus performance tests are designed (1) to measure intelligence primarily, and (2) to do so without making demands on the subject's ability to use language. His response, therefore, is almost of necessity manual, and it follows that the problem presented in a performance test is largely of a concrete nature. These tests, however, must not be confused with those of manual dexterity (in which success depends primarily on skill and deftness with the fingers), or with tests of mechanical ability

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[1] Itard, J. *De l'éducation d'un homme sauvage,* 2 pamphlets, 1801 and 1807.

[2] Séguin, E. *Idiocy and its Treatment by the Physiological Method,* 1866.

(in which recognition of mechanical relationships and principles is necessary for a successful solution). Performance tests are essentially measurements of intelligence, measurements in which the manual response is but a means to an end, analogous to the written response required in numerous linguistic tests.

2. *Causes to which the development of performance tests can be traced.*

The development of performance tests may be traced to three causes: the interest of psychologists and educationists in the nature of intelligence; the practical necessity for some non-verbal method of measuring mental ability; and the modern emphasis on individual differences.

Interest in the nature of intelligence has inspired a large number of researches in England, France, America and Germany, many of which can be considered as the experiments preliminary to performance tests. These experiments consisted in the measurement of various processes, and the comparison of these processes with independent criteria of mental ability. Chief among the qualities studied were those of a physical nature —such as reaction time, height, weight, strength of grip and sensori-motor processes, both simple and complex. Among the earliest investigations should be mentioned those of Galton[1] who in 1883 first attempted to measure intelligence by means of laboratory tests, and those of Cattell in collaboration with Galton[2]. There followed a number of experiments,[3] with both normal and abnormal adults, as well as with school children, conducted by numerous investigators, among whom might be named Porter[4], Smedley[5], West[6], Wissler[7], and Kelly[8].

In general the results of these experiments were somewhat contradictory. Bagley[9] in an elaborate study in 1901, and Perrin[10] twenty years later found an inverse correlation between

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[1] Galton, Sir Francis. *Enquiries into Human Faculty and its Development*, 1883.

[2] Cattell, J. McK. Mental Tests and Measurements. *Mind*, 1890, **15**, 9.

[3] See Report of Committee on Physical and Mental Tests, American Psychological Association, *Psychological Review*, **2**, 2.

[4] Porter, T. The Physical Basis of Precocity and Dullness. *Academy of Sciences*, St. Louis, U.S.A., 1903.

[5] Smedley, F. Physical Development and School Standing. *Department of Child Study and Research*, Chicago, U.S.A., 1900.

[6] West, G. M. Observations on the Relation of Physical Development and School Standing. *Science*, **4**, 3.

[7] Wissler, C. *Psychological Review, Monograph Supplements*, **3**.

[8] Kelly, R. S. *Psychological Review*, July, 1903.

[9] Bagley, W. On the Correlation of Mental and Motor Ability in School Children. *Am. J. of Psy.*, **12**, Jan., 1901.

[10] Perrin, F. A. C. An Experimental Study of Motor Ability. *J. of Expt. Psy.*, **4**, Feb., 1921.

mental and motor ability, while, on the other hand, Gilbert[1] in 1893 and Spearman[2] in 1904 found a positive correlation between various sensori-motor processes and intelligence. Burt, (1909[3]) as a result of correlating a group of mental and motor tests found little or no correlation between simple sensori-motor tests and general intelligence, although the correlation from some of the more complex sensori-motor tests, such as irregular dotting, card sorting and alphabet sorting, were high enough to prove them of undoubted value in the diagnosis of intelligence. The appearance of Binet's scale[4] of intelligence tests—resulting from his experiments with French children—gave rise to many researches in the evaluation and analysis of various qualities in relation to general intelligence. Among such researches should be mentioned Bobertag's[5] with German children, Burt's (1921) with London children, Goddard's[6], Kuhlman's[7], Terman's[8], Child's[9], and Yerkes'[10] with American children.

Considering the numerous experiments on the nature of intelligence, so far as they concern non-linguistic tests, the results are at first view disappointing. Not only are they contradictory, but between non-linguistic processes and criteria of general intelligence, correlations, if found at all, are, with a few exceptions, scarcely large enough to be significant.[11] Consequently, in the selection of measurements for general intelligence the emphasis

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[1] Gilbert, J. A. Researches on the Mental and Physical Development of School Children. *Studies from the Yale Psychological Laboratory*, **2**, 1894.

[2] Spearman, C. General Intelligence Objectively Determined and Measured. *Am. J. of Psy.*, **15.**

[3] Where a date is given after the name of an author quoted in the text, the reference will be found in the "Bibliography of Performance Tests" (p. 41)

[4] Binet, A. Le développement de l'intelligence chez les enfants. *L'Année Psychologique*, **14**, 1.

[5] Bobertag, O. Ueber Intelligenz Prufungen. *Zeit für Angew. Psych.*, **2**, 105 ; **5**, 105 ; **6**, 495.

[6] Goddard, H. H. Articles on the 1911 Revision of the Binet-Simon Scale. *Training School Bulletin*, 1911, 1913. Vineland, N.J., U.S.A.

[7] Kuhlman, F. A Revision of the Binet-Simon System for Measuring the Intelligence of Children. *J. of Psycho-Asthenics.* Monograph Supplement, 1912.

[8] Terman, L. M. *The Measurement of Intelligence* Houghton, Mifflin Co., 1916.

[9] Terman, L. M. & Childs, L., et al. *The Stanford Revision and Extension of the Binet-Simon Measuring Scale of Intelligence.* Warwick & York, 1917, Baltimore.

[10] Yerkes, R. M. Bridges, and Hardwick, R. *A Point Scale for Measuring Mental Ability.* Baltimore, Warwick & York.

[11] The types of non-linguistic tests which were used, and the criteria with which these tests were compared may account for this result. In any case, the possibilities for non-linguistic measurement were far from exhausted, and it must not be forgotten that these results apply only to certain types of non-linguistic tests.

has been overwhelmingly on those of a linguistic type, and the development of non-linguistic tests has correspondingly lagged behind.

It must not be supposed, however, that the results of the numerous researches referred to were entirely worthless so far as non-linguistic tests are concerned. Two valuable contributions were made ; first, directly light was thrown on various theoretical problems and, in a few instances, fruitful suggestions were supplied for actual performance tests ; and secondly, indirectly and in a negative way, the limits of the field in which non-linguistic tests must be developed, were indicated, so that, when devising such tests, we know that it is valueless to include certain measurements, as, for example, those of the simple sensory processes.

The second—and perhaps the principal—cause for the development of performance tests has been the practical necessity for some non-linguistic means of measuring intelligence. This necessity has forced psychologists and educationists to look further than the previously mentioned experiments on the nature of intelligence, and to find other tests of a non-linguistic character, tests which will correlate with general intelligence. Performance tests have been developed chiefly in America, where the necessity for measuring the intelligence of non-English-speaking foreigners, as well as the need, in common with other countries, for classifying mental defectives, has given a special impetus to the development of non-linguistic tests. Also the study in America and elsewhere of physically abnormal subjects—as the deaf, the blind, and persons with speech defects has doubtless aroused much interest in non-linguistic tests.

The third cause to which the development of performance tests may be traced is the comparatively modern interest in individual differences.[1] This interest has led to a consideration of the differences between individuals in methods of expression as well as in other respects. It has also led inevitably to a study of the individual as such. Consequently there has followed a reaction against the too exclusive emphasis on ability of a linguistic type, and an insistence on a study by non-verbal methods of the individual whose ability does not make itself evident in linguistic terms. In this connection the interest in performance tests can be taken as indicative of a general trend in psychology, a trend illustrated also by the appearance of various measurements of special abilities and of numerous non-linguistic group tests of intelligence.

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[1] A discussion of the question of individual differences as it affects psychology in general will be found in Prof. Cyril Burt's presidential address to the Psychological Section of the British Association, 1923. *See* " The Mental Differences between Individuals."

C.—Types, Principal Scales and Uses of Performance Tests.

(1) *Types of performance tests.*

The first performance test to be used as such was the formboard. Norsworthy[1] in 1906, employed one of Séguin's formboards in testing a group of mentally defective children. Among the next performance tests to be used were those of a "picture completion" type, tests in which a dissected picture is to be fitted together again to make an intact whole—a simplified form of which later became popular as a jig-saw puzzle. Burt (1911) and Healy and Fernald (1911), during the same year, published researches describing tests of this kind. Numerous other examples of these two types of tests, the formboard and the picture completion, have been developed since they were first used. Among other kinds of performance tests which may be mentioned specifically are those consisting of a maze out of which the subject must find his way (Porteus, 1922), and those dependent on the logical analysis of form relationships, in both two and three dimensions.[2] All these various types of performance tests may be classified, on the basis of the materials used, in one or the other of two ways: either as (1) manipulative, those in which the subject handles and rearranges actual objects, such as blocks of wood and weights; or as (2) non-manipulative, those in which he deals with pictures or other symbolical representations of the objects.

(2) *Description of principal scales of performance tests.*

The following does not profess to be a complete list of these tests. All that has been attempted is to mention the most widely useful scales, as well as some single tests (not part of any scale) of a performance type.

Three scales consisting of performance tests only, deserve special consideration; Pintner and Paterson's (1917) scale, Dearborn's (1923), and that used in the U.S. Army during the war (Yoakum and Yerkes, 1920). The first, which was published in book form in 1917, is perhaps the most widely used at the present time. It consists of fifteen tests of several different types. This work was a direct outcome of the authors' attempts to measure the intelligence of deaf children, although the scale has been standardized for normal individuals of both sexes with chronological ages ranging from three to sixteen years. Certain tests of Dearborn's scale—which has been worked out in conjunction with Shaw, Lincoln, Anderson and Christiansen—were published in 1916

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[1] Norsworthy, N. The Psychology of Mentally Deficient Children. *Archives of Psychology*, **1**, Nov., 1906.

[2] *Memoirs*, **15**. National Academy of Sciences. Washington, D.C., U.S.A.

(Dearborn, Anderson and Christiansen, 1916), and 1918 (Shaw, 1918). The complete scale with standards for normal boys and girls was published in 1923. It has been used extensively by the authors with normal subjects, as well as in studying children who presented some scholastic or disciplinary problem. The U.S. Army performance scale represents the pooled results of a number of psychologists' work on performance tests. It includes parts of both of the preceding scales, as well as various other tests. Although the Army scale was not available to the public until 1920, it was, of course, in use several years earlier, when a large number of non-English-speaking recruits in the American Army were tested during the war. No norms for this particular combination of tests are available except those based on soldiers' records, so that it represents results from a much more selected group than either Pintner and Paterson's or Dearborn's scale.

Four other series of tests, in which performance tests play an important part (although verbal tests are also included in these series), should be mentioned. First among these in point of time is Healy and Fernald's (1911) Tests for Practical Mental Classification. This scale, published in 1911, contains twenty-two tests, nearly two-thirds of which could be classified as performance tests. It was a result of a number of attempts on the part of the authors to find a satisfactory measure for the intelligence of juvenile delinquents, a measure independent of the subjects' formal education and ability to use English or any other language. Knox's (1914) scale, based on results obtained with immigrants at Ellis Island, New York, was published in 1914. Various formboards, as well as other types of performance tests, are included in this scale. In 1914 also, Woolley and Fischer's (1914) Monograph on Mental and Physical Measurements of Working Children was issued. The results from performance tests included in this scale are particularly interesting as they are used for vocational guidance of normal subjects. Haine's (1916) Point Scale for the Blind, published in 1916, contains some tests unique among performance tests in that touch is substituted for vision, while the subjects' response remains non-linguistic.[1]

When a survey is made of single performance tests, or of specific types of performance tests—as opposed to performance scales in which several types of tests are used—about a dozen should be noted. Chief among those should be named two groups of formboard tests; Ferguson's (1920) Graduated Series of Formboards, a series which is difficult enough at the upper

[1] A scale of tests containing several which would be classified as performance tests according to the definition here given has been devised and applied to London school children by Miss Margaret MacFarlane. The results obtained with this scale are now in the press (see "Studies in Practical Ability," *B. J. of Psych., Monograph Supplement*).

end to give a reliable indication of college students' mental ability, and Kent's (1916a) Series of Geometrical Puzzles, designed to test both children and adults. A series of block design tests—which has proved among the most satisfactory of performance tests for a wide range of ages—has been described by Kohs (1923), who has made an extensive investigation of this type of test and has standardized his series of block designs on a large number of individuals. One of the best known and most useful series of performance tests consists of the printed mazes devised by Porteus (1919). These mazes are of special value as a measure of certain temperamental qualities, as well as of intelligence. Porteus has used them with about 6,000 subjects, including normal as well as sub-normal and psychopathic individuals, and children of primitive races. The use of these mazes in connection with Porteus's Social and Industrial Rating Scales make them of especial interest. The Woodworth and Wells' Substitution Test, revised by Wells and known as Substitution Beta should perhaps be specially noted. This revision is apparently much more difficult than the original Substitution, and is of interest as it promises to contribute to the urgent need for non-verbal tests sufficiently difficult for adults.

A number of individual tests of the performance type, not part of any scale or definite series—such as card-sorting, mirror-drawing and cancellation—are described in Whipple's *Manual of Mental and Physical Tests* (1914).

It should be noted here that although some of the scales mentioned include a few tests for adults, tests which discriminate between normal persons at or above 16 or 17 years, there are very few performance tests (either parts of scales or single tests), sufficiently difficult to be used as a basis for comparing normal adults. It is true that the U.S. Army scale was used entirely for adults. The purpose of this scale, however, was largely to sort out men of inferior intelligence, rather than to compare those of normal mental ability.

The performance tests described in the foregoing paragraphs fall into three divisions: (1) those which are parts of scales, as Pintner and Paterson's; (2) those which are parts of a series all the members of which are of the same type, as Ferguson's series of formboards; and (3) those which are single tests, such as Woodworth and Wells' Substitution Beta, developed individually rather than as parts of a scale or series. A scale strictly so-called, comprises tests of a number of different manifestations of general intelligence; and in order to arrive at a mental age the total scale or a prescribed portion of it must be used. On the other hand, series of performance tests of one type and single tests cannot be taken as indicative of mental age for intelligence, just as a single test or a series of one type of test (for example, memory for

digits), from the Binet scale cannot be so used. Individual performance tests and groups of tests of one type have their principal value when used in conjunction with, and as supplementary to, a standardized scale of intelligence tests, or some other criterion of mental ability.

(3) *Uses of Performance Tests.*

The uses of performance tests may be summarised as follows :—

(1) To test foreigners, who speak little if any of the language in which the test is given, and who are often illiterate.

(2) To test subjects with physical abnormalities, which handicap them in a linguistic test. Among such subjects would be the blind and the deaf, both of whom can be given only parts, and in some cases none, of the Binet scale, for example. Speech-defectives are also frequently given performance tests rather than linguistic tests, since defectives of this type, even though they may be able to make verbal responses, are usually so self-conscious and uneasy while doing so as to influence their score.

(3) To test physically normal subjects, who present some special problem, with a method that shall supplement the result of the ordinary Binet or other linguistic scale, and shall give the most comprehensive view possible of the individual's intelligence. In bureaux of vocational guidance and in psycho-educational clinics devoted to the study of children presenting special scholastic or disciplinary problems, performance tests are widely used. In clinics for mental defectives, juvenile delinquents and psychopaths of various types also, performance tests are used as part of the routine procedure. Among the classes of subjects considered in these clinics, specific mention should perhaps be made of the type whom Healy (1915) has called " verbalist "; a type which is characterized by a capacity to use language very much above its ability in other ways. In a linguistic test, generally backward subjects with this special ability, may mistakenly be graded as normal.

(4) To test English-speaking persons, physically normal, with a series for tests not as a supplement to, but as a substitute for the Binet scale. This use of performance tests is adopted only to a very limited extent, usually in clinics for young children, where it is considered that performance tests arouse greater interest than do linguistic tests, and therefore are superior to them.

Where a non-linguistic method is necessary, or where examiner and subject speak different languages, there are in use but two alternatives to performance tests—either a non-language group scale of intelligence, or an ordinary linguistic test explained through an interpreter. The former method, although it is still much in its infancy, has great promise. It is not, however, likely to be entirely comparable to a series of performance tests, since there are obviously great difficulties in the way of a group scale including tests similar to manipulative performance tests; and hence group scales must almost of necessity be limited to non-manipulative tests. The more obvious alternative to performance tests, where examiner and subject speak different languages—the use of an interpreter—has serious drawbacks since the interpreter, unwittingly, is likely to help the subject, and since the presence of a third person may disturb the rapport between subject and examiner.

II. DESCRIPTION OF A SERIES OF PERFORMANCE TESTS.

A.—Descriptions of the Tests and Directions for Giving Them.

A series of 14 performance tests has been given in three elementary schools under the London County Council, and norms have been compiled for the use of these tests with English children. This group of tests[1], which is now to be described, comprises several from Pintner and Paterson's (1917) scale, and several from the U.S. Army Scale of Performance Tests (Yoakum and Yerkes, 1920). It includes, in addition, one other test, the Porteus (1919) Maze Test.

It should here be emphasized that this series of tests[2] has been arranged as a scale, and that it is intended that the complete

[1] This series of performance tests was arranged in 1921 by the psychologists at the Psychopathic Hospital, Boston, U.S.A., including the present writer. A manual of directions was also compiled at that time. The procedure in this manual for giving the tests was adhered to with the elementary school children ; the methods of scoring have been somewhat altered, as the tests have been partially re-standardized with English children.

[2] It is particularly desirable with the scale here described to use all tests prescribed for each age, since the 'reliability' of the scale as a whole is considerably higher than for some of the separate tests. ('Reliability' here and elsewhere is used in its technical sense. Tests are sometimes given twice to the same individuals, with an interval of time between the two testings. The extent to which the scores for each individual from the first testing accords with those from the second, is called the reliability of the test.)

series or a prescribed part of it be used.[1] To measure a subject's intelligence it is not sufficient to administer a single test, or even a number of tests selected at random. A scale of tests is indispensable.[2] Like all other tests, those constituting this series are based on the principle, familiarized by the Binet scale, of sampling a number of different abilities, and amalgamating the total results. To do this, it is essential to give a number of different tests and to combine the results in the prescribed way just as in the Binet scale.

It is not to be understood, however, that it is necessary in every case to give all the tests in this series in order to get a satisfactory rating; indeed, such a procedure would be a waste of time in some circumstances, as not all the tests discriminate between all mental ages. The range of the different tests is shown in Fig. 1. The shaded portions indicate those mental ages for which the tests are not suitable. For example, the Goddard Adaptation Board is not discriminative above 10, the Manikin is not above a mental age of 8, while the Cube Imitation Test has a much wider range of usefulness and can be given profitably to subjects with mental ages between 5 and 15. Thus it will be seen that the tests vary considerably in the range of mental years which they cover. Analogous variations appear in the Stanford-Binet scale where some types of tests are re-inserted at several

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[1] Further information than is given here, and records of detailed studies concerning the tests used in this series may be found in the following places :—

Porteus Test	Burt, C. (1921); Porteus, S.D. (1922).
Cube Imitation Test .. .. .. Adaptation Board .. .. .. Manikin Test .. .. Goddard Formboard .. .. .. Diagonal Test .. .. Woodworth & Wells' Substitution Test .. .. Triangle Test .. .. Healy Construction Test A .. .. Profile Test	Pintner, R. & Paterson, D. (1917).
Healy Picture Completion Test I ..	Pintner, R. & Anderson, M. (1917).
Cube Construction Test .. .. Dearborn Formboard .. .. .. Healy Picture Completion Test II .. Manikin Test .. .. Profile Test .. .. Woodworth & Wells' Substitution Test	Memoirs of the National Academy of Sciences. Vol. 15. Washington, D.C., U.S.A.; Yoakum, C. S. & Yerkes, R.M. (1920).
Dearborn Formboard	Dearborn, W. F. ; Shaw, E. A.; & Lincoln, E. A., (1923).

[2] Arrangements can be made for the supply of copies of the tests here described through the National Institute of Industrial Psychology, 329 High Holborn, W.C.1.

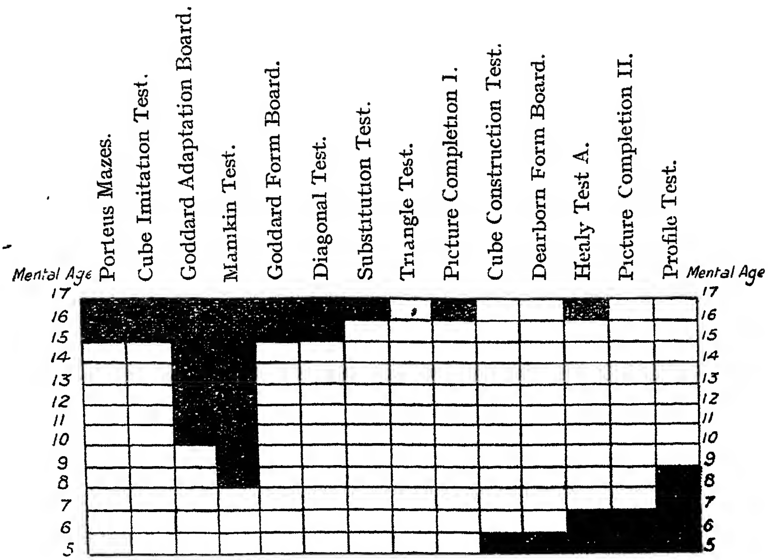


FIGURE 1.—Diagram for determining the performance tests to be used at various ages.

age-levels (for example, memory for digits), while others appear only at a few (for example, the fable, or the ball and field). In selecting the tests to be used for a given subject, his chronological age is first to be considered. Thus a child of 9 would be given all the tests except the Manikin because all the other tests discriminate at and above 9. Only if he did poorly on the other tests would he be given the Manikin.

(1) *Porteus Maze Test.*

Material.—The material for this test consists of eleven mazes printed on separate sheets. These mazes are graded progressively in difficulty.

Procedure.[1]—" For all children above the level of the infants' school, the examiner begins with the maze for age V. The child is told to ' suppose this is the plan of the paths in a garden. These lines are walls which you cannot get over. Start with the pen from the mark at the top, and find your way out of the garden by the quickest path. Show me, first of all, any openings that you can see.' (*The two openings are indicated.*) ' All the other paths are blocked. Don't

---

[1] The procedure and scoring and also the figures given concerning it in Tables A and B (*see* below) for this test are those used with English children by Prof. Cyril Burt, and described in *Mental and Scholastic Tests*, L.C.C., 1921.

go up any of the blocked turnings. Go down this path from the top, and then out by the *first* opening you can find.' To avoid marking the paper, the child uses a dry pen or a pointed stick. After he has once begun, he should not lift his pen from the paper.[1]

"With the succeeding patterns the openings are not pointed out first of all. Emphasize that the child must find his way out without turning up any of the blocked paths; and, as soon as he enters a blind alley and has discovered that he is blocked, do not allow him to correct his mistake by retracing his path, but bring him back to the starting point for a second trial. If for some reason—for example, a suspected accident in a successful second trial—a third attempt seems needed, invert the diagram and treat it as a new test.

"Dr. Porteus states that the tests need be continued only until the child entirely fails with two successive mazes. I suggest that, unless it is plainly evident that the remainder are too hard, each child should be allowed to attempt every test."

Scoring.—"The following method is recommended for scoring the results. One mark is awarded for each test correctly performed on the first trial. Half a mark only is allowed if a second trial is necessary for success, or if, in the test for year V. the child passes out of the second or lower opening. To the total number of marks thus gained, add four marks for presumable success in imaginary tests for ages I. to IV.; the result will give the child's score roughly in terms of a mental age. There is no test for age XIII. But four trials are allowed with the mazes for ages XII. and XIV. With that for age XII. a full mark is allowed for a success in any of the first three trials; and half a mark for a success in the fourth. But with that for age XIV. two marks are allowed for a success in the first trial; and for every further trial needed half a mark is deducted; thus a child who does not succeed until the second trial scores only one and a half, and so on.

"For English children the standardization does not appear altogether perfect. The percentage in each age-group, passing the tests assigned to their own year, differs much from maze to maze. For example (counting two half-successes scored by two different children as equivalent to one full success scored by a single child), only 62 per cent. of the children aged 5 pass the maze for age V., while as

---

[1] Dr. Porteus requires the child to mark his path visibly in pencil. This entails a fresh sheet for every child, and with the same child, after every error.

many as 84 per cent. aged 8 pass the maze for age VIII.; and, indeed, even by modifying the given mazes slightly, or selecting mazes afresh from a new and larger series, it would be difficult to hit upon an evenly graduated scale containing only one test to mark the exact median for each year. Hence, I suggest that the mazes as a whole be regarded as forming a single graded test-series, roughly increasing in difficulty rather than as marking definite mental ages."

(2) *Cube Imitation Test.*

Material.—Five black one-inch cubes are used for this test.

Procedure.—The examiner places four cubes in front of the subject, about two inches apart. With the fifth cube he then taps the blocks according to the first line-pattern given below, about one tap per second. The cube at the subject's left (which is numbered 1) is always tapped first. After each line-pattern the examiner places the fifth cube in front of the subject, about on a line midway between the third and fourth cubes, saying " Do that." The next line-pattern is then tapped, and so on till the subject fails in at least three successive patterns. The successive line-patterns are as follows :—

a.	1–2–3–4	*e.*	1–4–3–2	*k.*	1–3–1–2–4
b.	1–2–3–4–3	*f.*	1–4–2–3	*l.*	1–4–3–1–2–4
c.	1–2–3–4–2	*g.*	1–3–2–4–3	*m.*	1–3–2–4–1–3
d.	1–3–2–4	*h.*	1–4–3–2–4	*n.*	1–4–2–3–4–1

Scoring.—The score is the number of lines correctly reproduced.

(3) *Adaptation Board.*

Material.—The Adaptation Board is an oblong wooden board, through which four circular holes have been cut. Three of them are about 6·8 cm. in diameter and the fourth about 7 cm. in diameter. There is a wooden block which exactly fits the larger hole.

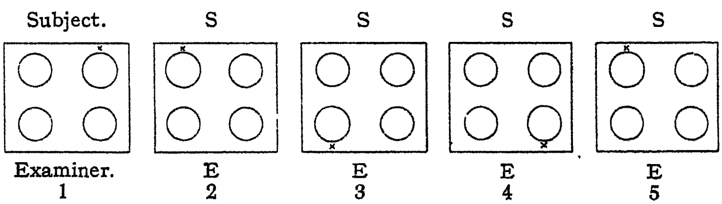


FIGURE 2.—Goddard Adaptation Board.
The position of the larger hole is indicated by a small cross.

Procedure.—The successive positions of the larger hole (marked x) are shown in Fig. 2. The board is first placed with the larger hole as in diagram at *examiner's upper right-hand corner.* The examiner, holding the board in his left hand and the

block in his right, then shows the subject (who should be seated opposite him) that the block fits in the larger hole only. The block is then given to the subject, with the direction to " Put it in the right hole." If he fails he is shown his mistake. The examiner then turns the board over from his right to his left so that the larger hole is in the position shown in diagram 2, at *examiner's upper left-hand corner*. The subject is asked to put the block in the right hole, and as before, if he fails, is shown his mistake. The board is then turned to positions shown in diagrams 3 and 4, with larger hole at *examiner's lower left-hand corner*, and at *examiner's lower right-hand corner* respectively. If the subject fails in either case he is shown his mistake. In each problem the board is turned over, and for the first four problems the long axis should be horizontal to the table. For the last move the examiner takes the board in his right hand, and turns it up and away from himself until the short axis is vertical to the table. He then turns it anti-clockwise about the centre as an axis until the larger hole is at his *upper left-hand corner*. He then turns the board a third time, so that the short axis is again horizontal to the table as at the beginning of this problem. (*See* diagram 5.) Each change of position is made with an even movement, in about one or two seconds.

Scoring.—The score is the number of correct first placings of the block.

(4) *Manikin Test.*

Material.—This test consists of pieces of wood made to represent a man's arms, legs, trunk and head.

Procedure.—The pieces are placed in the following order (going from the subject's left to right) :—the left arm, the left leg, the trunk, the right leg, the right arm, the head. The examiner does not tell the subject what the pieces represent but merely says " Put this together as quickly as you can."

Scoring.—The score is recorded in points, as follows :—

A complete performance, absolutely accurate—5 points.

One or both arms up or out, i.e., not exactly fitting in the joints—4 points.

One reversal, i.e., right arm for left arm and vice versa, or right leg for left leg—3 points.

Two reversals, i.e., both arms and both legs reversed—2 points.

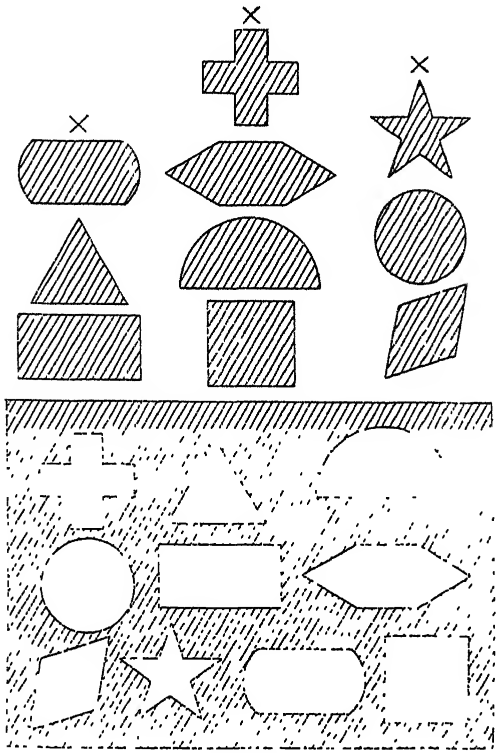
Legs or arms interchanged or arms at sides, or any other result which looks like a man—1 point.

Failure to see that it is a man—0 points.

5. *Goddard Formboard.*[1]

Material.—The material consists of a wooden board (13 in. × 18 in.) and ten wooden blocks which fit the recesses cut in the board.

Examiner.



Subject.

FIGURE 3.—Goddard Formboard.

The three pieces marked with crosses are in the bottom layer.

Adapted, by permission of the publishers, from Pintner and Paterson : " A Scale of Performance Tests." D. Appleton & Co., New York and London. 1917.

Procedure.—The material is placed as shown in the diagram, except that the pieces are piled on top of each other. The table on which the board is placed must be such that the subject can lean over the board and look down on its centre. A child should usually be asked to stand while doing this test. The subject is told to put the blocks into place as quickly as possible. He is urged and encouraged if necessary. Three trials are given, no one of them being allowed to last over five minutes.[2]

[1] This test has been revised by Goddard from one of Séguin's Formboards.

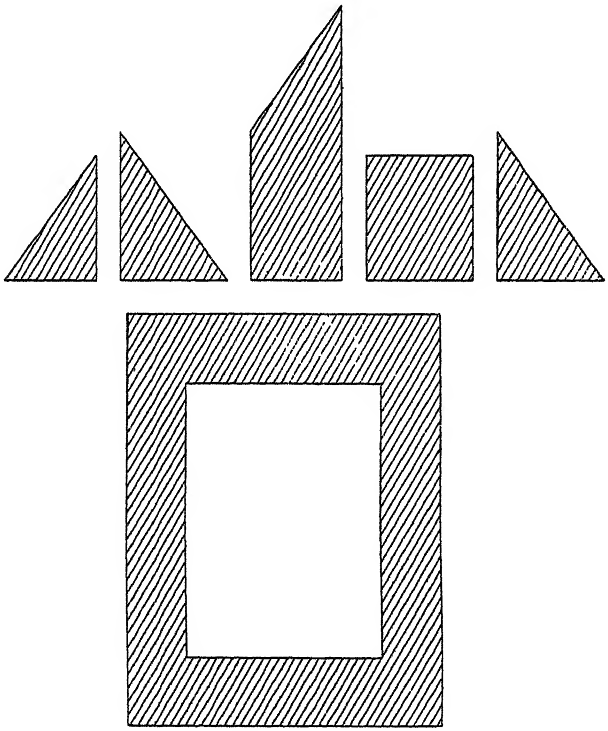
[2] As time is an important factor in the scoring of so many of the performance tests, it should be emphasized that a warning, followed by a definite signal for beginning, should always be given to the subject. Some such expression as " Ready—Go ! " may be used.

Scoring.—The score is the time taken for the shortest trial. No credit is given unless all the blocks are correctly placed. Another method of scoring is that of using the average time of the three trials. This method, however, has not been fully tried out as yet, but some use of it has been made in this Report (*see* Table B. p. 36).

6. *Diagonal Test.*

Material.—This test consists of a wooden frame 5 in. × 6·5 in. and five insets to fit into this frame.

Examiner.



Subject.

FIGURE 4.—Diagonal Test.

Adapted, by permission of the publishers, from Pintner and Paterson: "A Scale of Performance Tests." D. Appleton & Co.: New York and London. 1917.

Procedure.—The material is placed as in diagram. The only directions given are "Put this together as quickly as you can."

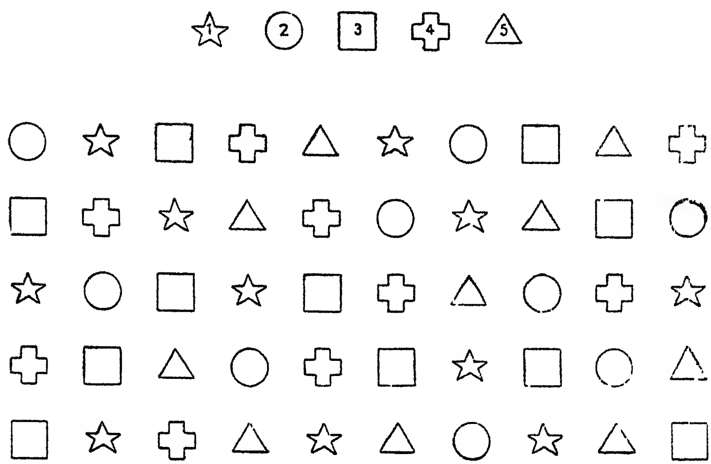
There is a time limit of 4 minutes 35 seconds.

Scoring.—The score is the number of seconds taken to place the insets correctly. No credit is given for a partially correct solution.

7. *Woodworth and Wells' Substitution Test.*

Material.—A printed test sheet and a pencil are the materials needed.

Examiner.



Subject.

FIGURE 5.—Substitution Test.

Reproduced by permission of the publishers, from Pintner and Paterson : " A Scale of Performance Tests." D. Appleton & Co. : New York and London. 1917.

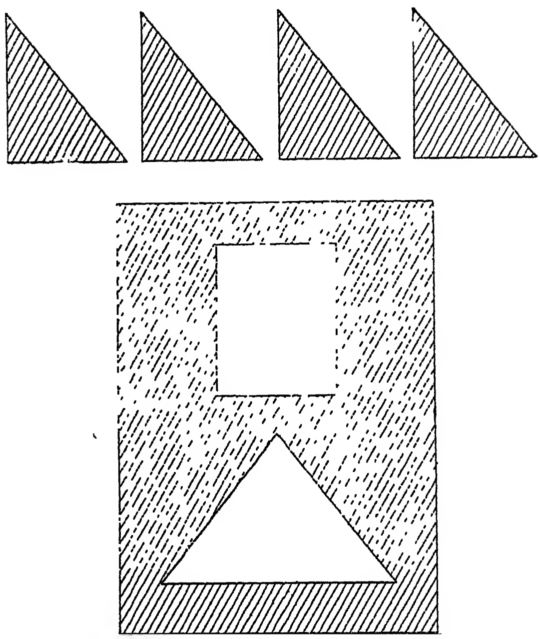
Procedure.—The test sheet (*see* Fig. 5) is placed before the subject and his attention is called to the key at the top. The examiner says " There are numbers in these figures at the top (*pointing to the key*), but there are no numbers in any of these (*pointing to the five rows*). You are to fill in these (*pointing again to the five rows*) with the right numbers. Look at the key and say which number goes here, for example (*the examiner points to first figure*)." If the subject does not understand at once the explanation is repeated until it is reasonably sure that he does. The examiner then says : " Fill in these figures with the right numbers beginning here (*pointing to first figure*), and work along each line, across the page from left to right." The subject is not allowed to fill in all the figures of one sort, as, for example, the star, then all of another and so forth. He must take them one after another as they come.

Scoring.—The total time in seconds is recorded and also the number of errors. For the score there is added to the total time $\frac{1}{50}$ (one-fiftieth) of itself for each error.

8. *Triangle Test.*

Material.—This test consists of a wooden frame and four right-angle triangles. The frame is similar to that used in the Diagonal test, and measures 5 in. × 6·5 in.

Examiner.



Subject.

FIGURE 6.—Triangle Test.

Adapted by permission of the publishers, from Pintner and Paterson : " A Scale of Performance Tests." D. Appleton & Co. : New York and London. 1917.

Procedure.—The material is placed as in the diagram, the examiner saying : " Put this together as quickly as you can." There is a time limit of 4 minutes 35 seconds.

Scoring.—The score is the number of seconds taken to place the insets correctly. No credit is given for a partially correct solution.

9. *Healy Picture Completion Test I.*

Material.—The materials are a large picture mounted on wood and measuring 11 in. × 15 in., in which ten holes have been cut, each removing some essential object, and a box of 50 (fifty) smaller pictures, each one of which is mounted on wood.

Procedure.—The large picture is placed before the subject, with the box of small pictures where it can be seen and reached easily. The examiner should make sure that the small pictures which are needed in the solution are scattered throughout the box, and that no two of them are beside each other. He then gives the following explanation : " Look

at this picture and see what is happening. See what the people are doing. You are to fill in these empty spaces so as to make the picture look right, so as to make the best sense. Any of these blocks will fit into any of these spaces. But choose those which seem best to you, those which will make the best picture." (*Pointing to the wagon.*) "What is the man looking for? What is gone?" If the answer is "wheel," the examiner says: "That's right. Find the wheel among the other blocks and put it in." If the correct response is not given the examiner explains the situation more fully and tells the subject to find the wheel and put it in. The final directions are: "to fill in the other holes in the same way as carefully and quickly as you can." Speed is not to be emphasized, but should be mentioned. When every space is filled and the subject indicates he has finished, the examiner says: "Now look it over carefully. See if you are satisfied with every block. See if it is exactly as you want it, and then tell me when you have finished." The time limit is ten minutes.

Scoring.—The table[1] below shows what marks are to be given for each picture inserted by the child. The score for the whole test is the total of the marks awarded for each picture placed.[2]

1. *Basket* ..	55
Bucket ..	2
Cherries ..	7
2. *Broken Window.*	100
Blank ..	2
Cage ..	1
Curtained Window	32
3. *Cat* ..	81
Baby ..	4
Chicken ..	2
Cup ..	1
Departing Cat.	7
Flying Bird	2
Fruit ..	1
Milk Bottle	4
Still Bird ..	1
Sleeping Cat	2
Stool ..	1
4. *Chicken* ..	58
Baby ..	1
Cage ..	1
Cat ..	2
Cherries ..	2
D. Cat ..	1
F. Bird ..	1
Hatchet ..	1
Mouse ..	1
S. Bird ..	2
5. *Dog* ..	64
Baby ..	2
Blank ..	2
B. Window	1
Cat ..	2
D. Cat ..	2
Hatchet ..	1
Mouse ..	1
S. Bird ..	1
Stool ..	1
6. *F. Bird* ..	87
Basket ..	2
Cage ..	7
Cherries ..	3
S. Bird ..	18
7. *Football* ..	84
Baseball ..	21
Cherries ..	2
F. Bird ..	1
Pumpkin ..	1 or 84
8. *Hat* ..	65
Baby ..	3
Books ..	1
Cat	2
Chicken ..	1
Dog ..	1
F. Bird ..	1
Mouse ..	2
Purse ..	3
9. *Log* ..	52
Blank ..	1
Hatchet ..	6
Stool ..	2

[1] The marks given in the table for various placings of the pictures were determined after an elaborate study of the test, and a careful consideration of the relative values of the small pieces. (*See* Pintner, R. and Anderson, M. (1917)).

[2] Full credit is given for placing the pumpkin (*see* item 7 in the table) where the football should be, if the examiner, after questioning the subject, is convinced that he took the pumpkin for a football. This change in the scoring was made after consulting Dr. Healy, the author of this test. It seemed no more than fair since very few of the London children to whom these tests were given had ever seen a pumpkin and since the one shown resembles a football.

10. *Cube Construction Test.*

Material.—The materials for this test are three wooden blocks called models 1, 2 and 3 respectively (*see* Fig. 7); and twenty-six 1-in. cubes necessary to construct these three models. Models 1 and 2 are 1 in. × 3 in. × 3 in. and are made to represent composites of nine small cubes. Model 1 is painted on the sides, but not on upper or lower surfaces; model 2 is painted on the sides and on the top surface as well. Model 3, a 2-in. cube made to represent a composite of eight small cubes, is entirely unpainted. The twenty-six small cubes are painted on some of their surfaces and unpainted on others, so that when properly put together they duplicate exactly the painted surfaces of models 1, 2 and 3 respectively.

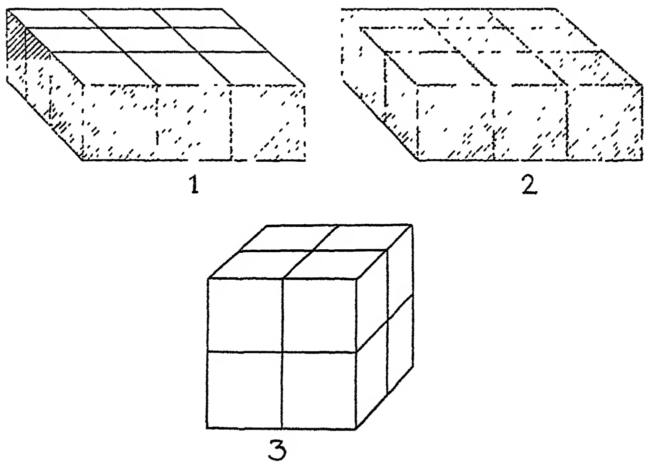


FIGURE 7.—Cube construction test.

Shading indicates portions which are painted red.

Procedure.—Examiner presents model 1, and says: "You see this block. Notice that it is painted on the sides, but not on the top or the bottom; and you see these smaller blocks, partly painted and partly unpainted. These nine blocks can be put together so as to make one just like this." Examiner puts the blocks together, pointing out and commenting on the painted surface or surfaces of each cube as he fits it in position. He then points to the cube which he has assembled and says: "You see that I have put these cubes together to be just like this big block (*pointing to model* 1). This one I have made (*examiner picks up the nine cubes which he has assembled and holds them together in his hand as one block*

while he points to the four sides, top and bottom, as he mentions them), is painted on all four sides, but not on the top and not on the bottom. You see it is just like this one " (*pointing to model* 1).

Examiner then presents the same model, and blocks in irregular order and says : " Now, you are to fit the blocks together so as to make one just like this."

Examiner presents model 2 and the blocks for its construction and says : " Now, put these blocks together so as to make one just like this. Notice that it is painted on the edges and on the top, but not on the bottom." (*Examiner points to the edges and the top and the bottom as he mentions them.*)

He then presents model 3 and says : " You see this block ; notice that it is not painted anywhere." (*He turns the block over and indicates that it is entirely unpainted.*) "And you see these smaller blocks that have three sides painted and three not painted. Now I want you to fit these eight blocks together so as to make one just like this. Remember, it is not painted on the bottom, top or sides."[1]

Scoring.—A record is made of the number of moves, i.e., placements in some position designed to complete or alter the structure. If parts of a structure are assembled separately, putting such parts together to form the structure as a whole does not count an additional move. If the blocks are fitted together in the hand, the moves are counted just as they are if the blocks are assembled on the table. Changing the face of a block, however, does not count as a move.

Time in seconds for reconstructing each model is also recorded. The time limit for work on each model is two minutes. If the subject assembles the blocks before the time is up, spontaneous corrections are allowed, counting the extra time and the additional moves. Each block changed counts one move as before. The time should be taken when the subject indicates verbally or otherwise that he has finished.

Whether the subject has finished or not, each misplaced block is counted as three additional moves ; each unassembled block six additional moves.

[1] In practice it has been found convenient to have separate boxes or envelopes for the blocks making up each of the three models. This not only saves time in giving the test, but avoids the possibility that the subject may be unduly assisted by watching the examiner sort the blocks for each part.

No mark is allowed for time if the blocks are not all assembled. Time (if the blocks are all assembled), and the total number of moves (whether the blocks are all assembled or not) are credited as follows :—

Models 1, 2. Moves.		Points.		Model 3. Moves.	Model 1. Seconds.		Points.		Models 2, 3. Seconds.
9	..	5	..	8	1–10	..	5	..	1–20
10–11	..	4	..	9–10	11–25	..	4	..	21–30
12–15	..	3	..	11–15	26–50	..	3	..	31–50
16–25	..	2	..	16–25	51–80	..	2	..	51–80
26–50	..	1	..	26–50	81–120	..	1	..	81–120

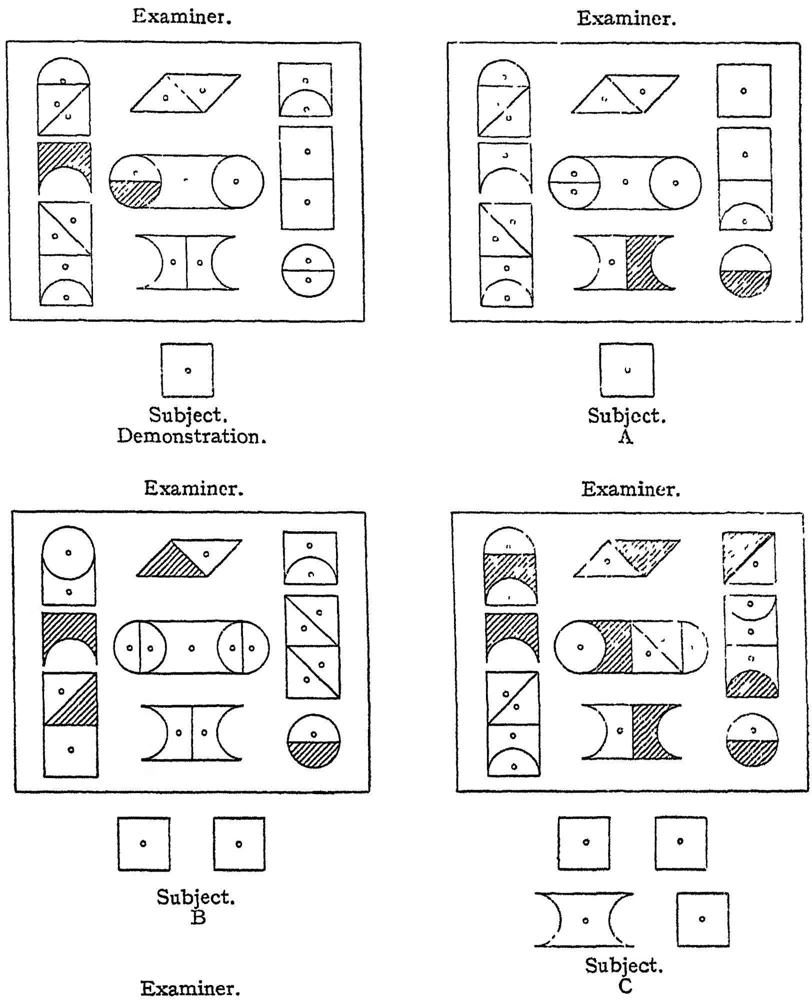
The marks for moves and for time are added for the total score.

11. *Dearborn Formboard.*

Material.—The material for this test consists of a formboard measuring 11·5 in. × 13·5 in., and six different types of insets, as illustrated in the diagrams.

Procedure.—The board is placed before the subject, arranged as in the diagram for demonstration. The examiner explains to the subject that the blocks can be re-arranged so as to make room for the extra square. The examiner then solves the problem, using the minimum number of moves, while the subject watches. A move is placing or trying to place a block in one of the apertures. The board is next presented arranged for problem A (*see* Fig. 8), the examiner saying, " Change these blocks so that you can find a place for the extra square (*pointing to the square*). Don't have any blocks left over, and do it in as few moves as you can." If not solved, the correct solution is demonstrated before going on to the next part. The board is then presented ready for problem B, the examiner telling the subject to re-arrange it so as to make room for the extra square. The board arranged for problem C is then presented, the examiner telling the subject again to make room for the extra blocks. The subject is not allowed to watch while the board is being re-arranged. A cardboard screen which can be placed between the child and the examiner is useful for this test. A copy of the four accompanying diagrams can be pasted on the side of the screen facing the examiner; and, with a little practice, the board can be arranged for the successive problems within 30 seconds.

The time limit for problems A and B is 2 minutes; for problem C it is 3 minutes.



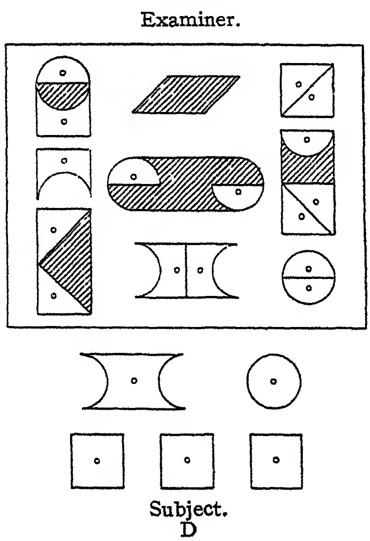


FIGURE 8.

Shaded parts indicate unfilled spaces.

Demonstration and Problems A, B and C are reproduced, by permission of the publishers, from Yoakum and Yerkes; "Mental Tests in the American Army." Henry Holt & Co., New York; Sidgwick & Jackson, Ltd., London, 1920. Problem D from Dearborn, Shaw & Lincoln: "A Series of Form Board and Performance Tests of Intelligence." Haward Monographs in Education, Series I, No. 4, 1923.

Scoring.—The time, in seconds, and the number of moves, taken for each problem separately, is recorded. If a problem is not solved within the time limit, that part of the test is scored zero; but if a correct solution is found, the problems are scored as follows:—

	Moves.			*Time.*		
Problem A.	B.	C.	Credit.	A. and B.	C.	Credit.
		8	5	0–10	0–20	5
		9	4	11–20	21–40	4
3	5	10–11	3	21–40	41–70	3
4	6	12–14	2	41–70	71–110	2
5–7	7–10	15–20	1	71–120	111–180	1

11a. Dearborn, in his scale of Formboard and Performance Tests,[1] uses a slightly different procedure and method of scoring this test from those just given. His methods are included here as they have been used largely with children, while the methods given in the preceding paragraphs have been used principally with men in the United States Army.

Material.—In Dearborn's scale, problems A, B and C are identical with A, B and C as given in the United States Army scale, except for a slight alteration: the board is turned round so that the positions of examiner and subject are reversed. Problem D, which is used in addition to A, B and C, is given as in the diagram.

The procedure and scoring for this test are quoted from Dearborn's account of his scale.

Procedure.—"Problem A is used as an example. After the blocks are arranged, the examiner says to the subject, 'I am going to show you how to do this puzzle, and then I will give you another like it to do yourself. Do you see these holes or spaces in the board (*pointing to them*) which have no blocks in them, and this square block which has been left out? What we want to do is to make a space or hole for this square block and put the block into it in such a way that all the holes in the board will be filled and no block will be left out. In order to do this we may change the other blocks about; only we must not make any more changes than are necessary.' The examiner then illustrates by filling the two empty spaces so as to clear a square space, in which he then places the square block.

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[1] *See* Account of this Scale on p. 8.

"The board is then concealed from the subject and set up for problem B as shown in the diagram. When it is ready the examiner presents it and says to the subject, 'I want you to place these two squares in the board so that all the holes will be filled and no blocks left out. Make just as few moves of the other blocks as you can, and work quickly.'

"Problem D is next presented in the same way, and problem C is given last, since it has proven more difficult than D.

Scoring.—"Each problem is scored for both time and moves. If a problem is not solved in three minutes, it is recorded as 'incomplete,' and the examiner passes to the next problem. . . . It is considered better to help the failing subject to find the solution quickly after the time limit is up rather than to break off suddenly, and thus emphasise his failure.

"A move is counted whenever the subject places or attempts to place a block in an aperture of the board. If the block is one which will fit in the depression, that is, if there is no error of form, the move is counted as correct, no matter if it does not contribute directly to the solution of the problem. The removal of a block from the board does not count as a move, since the move is made when the block is replaced on the board. A move is recorded as incorrect when the subject attempts to put a block into a depression which it will not fit. Right and wrong moves are added to get the total."

The tentative standards for this formboard as given by Dearborn are as follows:—

"A 'performance age' is first determined by the following table, according to the total number of moves made in doing the three problems.

P.A.	6	7	8	9	10	11	12	13	14
Number of Moves ..	48	45	42	39	36	34	32	28–31	22–27

"To the 'performance age' thus found add 1 year if the total time taken for the three problems is 4 minutes or less. If the performance age is 8 or above on the basis of the number of moves, and if the total time taken is over $5\frac{1}{2}$ minutes, subtract 1 year; if the total time taken is over $6\frac{1}{2}$ minutes, subtract 2 years.

"The number at each age, however, is small, and in the 6th, 9th, 10th and 13th years especially the medians are not reliable on this account. Further, it is probable that the six-year-olds are a somewhat superior group, and the thirteen-year-olds are probably inferior."

12. *Healy Construction Test A.*

Material.—This test consists of a wooden frame and five rectangles. The frame, like the Diagonal and Triangle Tests, measures 5 in. × 6·5 in.

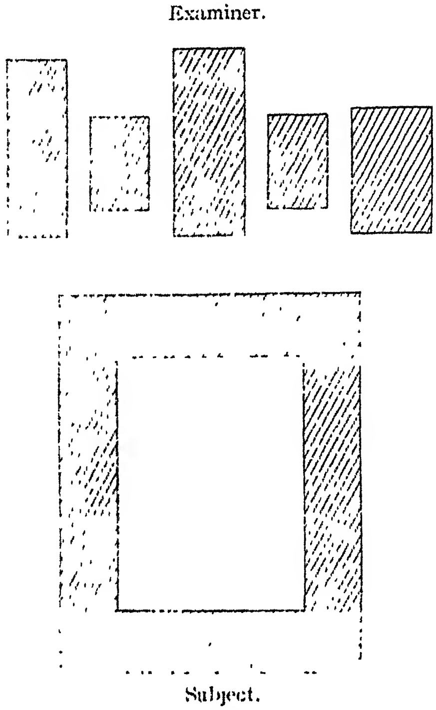


FIGURE 9.—Healy Test A.
Adapted, by permission of the publishers, from Pintner and Paterson: "A Scale of Performance Tests."
D. Appleton & Co.: New York and London. 1917.

Procedure.—The material is placed as in the diagram, the examiner simply saying, "Put this together as quickly as you can." There is a time limit of 2 minutes 11 seconds.

Scoring.—The score is the number of seconds taken to place the insets correctly. No credit is given for a partially correct solution.

13. *Healy Picture Completion Test II.*

Material.—For this test the materials are a large picture mounted on wood and measuring 11 in. × 11·5 in., and sixty small pictures also mounted on wood. This picture is divided into eleven sections, representing as many different scenes in a school boy's day. In each picture a hole has been cut removing some essential object.

Procedure.—The examiner places the large picture in front of the subject and the box of small pieces at the side where they can be seen and reached easily. Before the subject begins the test, the examiner must make sure that the small pieces are in their proper order in the box, i.e., in the order determined by the numbers printed on the back of each piece. The large picture is usually made in two pieces. The examiner must be sure that these are placed together and that the demonstration picture comes at the subject's left. The examiner then says: "Here is a story told in pictures. The story begins here" (*pointing to demonstration*) "where the boy is getting dressed. It shows the *same* boy, remember—the *same* boy, doing one thing after another during the same day." (*Examiner points along the first row, then along the second, to indicate sequence clearly.*) "You see in each picture a piece is missing. Here" (*pointing*) "are a lot of small pieces. They fit in any of the spaces, but there are more pieces than you can use. You are to pick out the piece that you think is needed, that is best to complete the sense of the picture. For instance, what is missing here?" (*pointing to demonstration*). "Yes, a boot." (If incorrect answer is given the examiner says: "No, he is dressing, and he is stooping for his other boot.") Examiner continues: "Now, which is the boot that he must have?" If the correct answer is given, the Examiner says: "Yes. This" (*pointing to shoe*) "wouldn't be right, because he must have a boot to match the other one. That is the way each picture is to be done. There is always some piece that is the very best one. You can tell which it is by studying the picture. Now go ahead."

The examiner gives no more help; and says nothing further except, if subject asks, to tell him he may change his moves. When all the spaces are filled, the subject is asked if he is quite satisfied. If not, he is told he may make changes.

The time limit is twenty minutes.

Scoring.—The accompanying table is used in scoring. The final placings only are considered, and the points of credit for each of these placings are read from the table. All placings which are not scored in the table (i.e., those which represent marked absurdities) are counted as minus 5 (—5). For example, piece 19 in Test 1 would be absurd. Hence we find that in the column headed 1 the space opposite 19 is empty, so that if this piece is put into this picture it is to be scored minus 5. For the total score the minus values are subtracted from the plus. The value representing the best piece for each picture is italicised in the table.

TABLE FOR SCORING HEALY P.C. II.

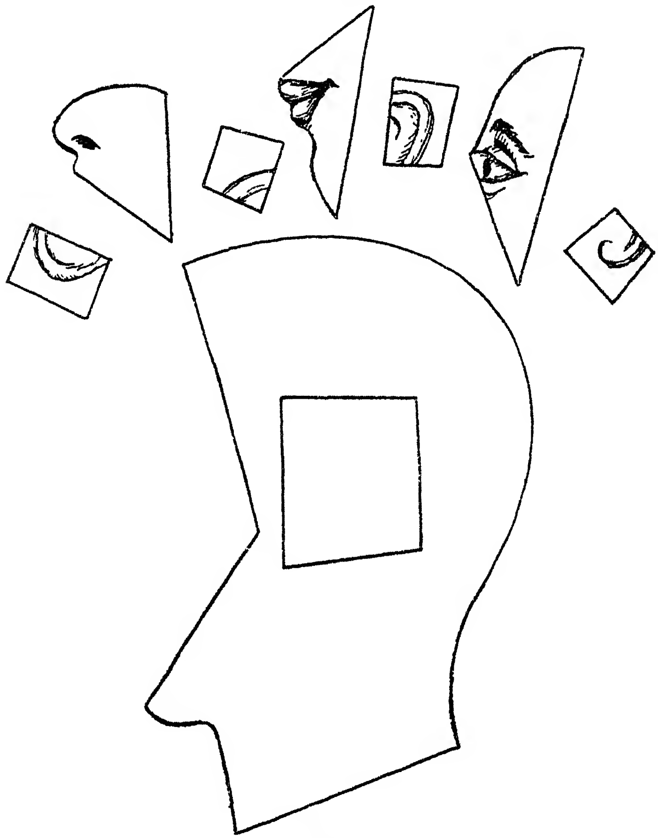
Value of Pieces in Pictures.

Pieces.	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	IX.	X.
1.. ..	–	–	–	–	2	–	–	–	–	*12·5*
2.. ..	0	0	1	2	–	0	0	0	0	0
3.. ..	–	–	–	–	–	–	1	*15·5*	–	–
4.. ..	–	–	–	–	0	–	–	–	–	6
5.. ..	–	–	–	–	–	–	0	0	0	–
6.. ..	0	–	–	0	–	–	–	–	–	–
7.. ..	0	0	1	2	–	0	0	0	0	0
8.. ..	*6*	–	–	2	–	–	–	–	–	–
9.. ..	–	5	0	–	0	–	–	–	–	0
10.. ..	0	–	–	1	–	–	–	–	–	–
11.. ..	1	–	–	8	–	–	–	–	–	–
12.. ..	–	0	–	–	0	–	–	–	–	1
13.. ..	–	5	0	–	3	–	–	–	–	1
14.. ..	–			–	–	–	1	6	–	–
15.. ..			1	–	–	1	0	0	0	–
16.. ..		–	–	–	–	–	0	0	0	–
17.. ..		–		–	–	–	1	6	–	–
18.. ..	–	–	0		–	*9·5*	–	0	–	–
19.. ..	–	2	–	–	0	–	–	–	–	0
20.. ..	–	–	–	–	–	–	–	–	–	–
21.. ..	–	0	–	–	1	–	–	–	–	0
22.. ..	–	–	–	–	2	–	–	–	–	6
23.. ..	1	–	–	*18*	–	–	–	–	–	–
24.. ..	–	–	–	–	0	–	–	–	–	6
25.. ..	–	–	–	–	–	–	2	0	0	–
26.. ..	–	–	0	–	–	4	0	0	0	–
27.. ..	–	5	0	–	1	–	–	–	–	0
28.. ..	–	–	0	–	–	4	0	0	0	–
29.. ..	–	–	–	–	–	–	–	–	–	0
30.. ..	2	–	–	2	–	–	–	–	–	–
31.. ..	1	–	–	8	–	–	–	–	–	–
32.. ..	–	0	–	–	*7*	–	–	–	–	0
33.. ..	–	–	–	–	–	–	1	7	–	–
34.. ..	–	–	–	–	–	–	*5·5*	0	0	–
35.. ..	–	–	*11*	–	0	0	–	–	–	0
36.. ..	–	–	1	–	–	2	0	0	0	–
37.. ..	–	0	4	–	0	0	–	–	–	0
38.. ..	–	–	–	–	–	–	–	–	–	0
39.. ..	–	–	–	–	–	–	0	0	1	–
40.. ..	–	–	–	–	–	–	–	–	–	1
41.. ..	–	0	–	–	2	–	–	–	–	0
42.. ..	0	–	–	2	–	–	–	–	–	–
43.. ..	–	0	–	–	2	–	–	–	–	0
44.. ..	–	–	1	–	–	1	0	0	0	–
45.. ..	3	–	–	2	–	–	–	–	–	–
46.. ..	0	0	6	0	0	0	–	–	–	0
47.. ..	–	5	0	–	1	–	–	–	–	2
48.. ..	–	–	–	–	–	–	0	0	*9*	–
49.. ..	–	*10*	0	–	1	–	–	–	–	2
50.. ..	0	–	–	–	–	–	0	0	1	–
51.. ..	0	0	1	2	–	0	0	0	0	0
52.. ..	1	–	–	8	–	–	–	–	–	–
53.. ..	0	0	1	2	–	0	0	0	0	0
54.. ..	–	–	–	–	–	–	0	0	0	–
55.. ..	–	–	6	–	0	0	–	–	–	0
56.. ..	–	–	–	–	–	–	2	0	0	–
57.. ..	–	–	–	–	–	–	0	0	1	–
58.. ..	0	0	1	2	–	0	0	0	0	0
59.. ..	–	–	–	–	–	–	0	0	2	–
60.. ..	–	0	2	–	0	0	–	–	–	0

14. *Feature Profile Test.*

Material—This test consists of pieces of wood which, when correctly put together, represent a man's face in profile.

Examiner.



Subject.

FIGURE 10.—Profile Test.

Adapted by permission of the publishers, from Pintner and Paterson : " A Scale of Performance Tests." D. Appleton & Co. : New York and London. 1917.

Procedure.—The pieces of the test are placed before the subject, as in the diagram. The examiner simply says " Put this together as quickly as you can." He does not tell the subject what the pieces represent, and if the position of the head is changed by the subject, the examiner must not place it in its correct position again. The time limit is four minutes.

Scoring.—The score is the time in seconds to assemble the pieces correctly. No credit is given for a partially correct solution.

B.—Interpretation of Results with the Tests.

In this section two kinds of results with performance tests are to be considered: first, those which can be stated numerically and are concerned principally with the scoring of the tests; and second, those which cannot be stated numerically, and are concerned largely with the temperamental factors which appear to modify a subject's scores. These two kinds of results might be spoken of as quantitative and qualitative respectively.

1. *Scoring.*

The scoring of the separate tests has already been described. The next step is to combine the scores from each of the tests so as to get a mental age and an intelligence quotient for every subject tested. Only a combination of the results from a number of tests is of practical significance; and the score in any single test, or in two or three tests, cannot be considered sufficient. That a given subject receives a score two years higher than that received by most individuals of his age in the Cube Construction Test, for example, is a fact of interest and of importance; it alone, however, is no indication whatever that his mental age is two years higher than his chronological age. As mentioned above, it is only on the basis of results from a group of tests that a mental age can be determined.

Table A has been made for the translation of scores in the individual tests into terms of mental age. This table has been constructed from scores made by English children, supplemented, in certain tests, by scores made in America.[1] Certain scores have been found to represent the medians for certain years, and are therefore assumed to be convertible into the corresponding mental ages. Thus a given score in any one of the performance tests may be found in the table and translated into terms of mental age. For example, it will be seen that a score of 5 in the Cube Construction test is equivalent to a mental age of 8·0. The two middle points between the median for each age and the median for the ages immediately above and below it mark the limits of the range for a given mental age. For example, in the Substitution Test, the median score for 8-year olds was found to be 158, for 7-year olds 180, and for 9-year olds 141. The middle points between year 7 and year 8, and between year 8 and year 9 are 169 and 149·5, respectively; and the mental age of a subject whose score is between these two points is 7·5, 8 or 8·5, according to whether his score is nearest the median for 8, the mid-point between 7 and 8 or the mid-point between 8 and 9. Where the same median score is assigned to two successive years, as in the Manikin test

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[1] With the exception of the Porteus Test, the English scores are given for years 10 to 13 inclusive, the American for the other years. In the Porteus Test, all results given are English, except for years 5, 15 and 16. *See* footnote 1, p. 12, for references to the American standardization.

TABLE A.

A Table of Norms for Translating Scores in Performance Tests into Mental Ages.

Tests. *Mental Ages.*	5	6	7	8	9	10	11	12	13	14	15	16
1. Porteus Maze Test	5	7	8	9	10	10	11	12	13	14	14	14
2. Cube Imitation Test	2	4	5	6	6	7	7	7	7	8	8	8
3. Adaptation Board	2	4	4	4	4	5	5	5	5	5	5	5
4. Manikin Test	3	4	4	5	5	5	5	5	5	5	5	5
5. Goddard Formboard—												
1st method (quickest of three trials) ..	37	26	23	20	18	16	15	14	12	11	11	11
2nd method (average of three trials) ..					*	20	17	16	12	—	—	—
6. Diagonal Test..	275	150	76	75	54	54	46	42	36	25	25	25
7. Substitution Test	600	290	180	158	141	110	100	99	98	97	96	96
8. Triangle Test	275	108	77	64	58	55	47	40	40	37	37	35
9. Picture Completion Test I	107	156	239	338	410	511	544	573	587	600	646	646
10. Cube Construction Test	—	2	3	5	7	9	10	10	11	17	18	20
11. Dearborn Formboard—												
1st method (as in U.S. Army)	—	5	7	9	11	13	15	17	18	18	20	21
2nd method (as in Dearborn's scale) ..					*	11	11	11	—	—	—	—
12. Healy Construction Test A	—	—	131	117	86	77	57	45	38	35	30	30
13. Picture Completion Test II	—	—	9	27	37	50	58	60	62	62	64	66
14. Profile	—	—	—	—	240	160	116	97	76	—	—	—

* These two rows are based on English scores; the row immediately above each, i.e., the figures referring to "1st methods," are based on American scores. The English score for year 13, 1st method, in the Dearborn Test is 15.

for years 6 and 7, the mental age of a subject who gets this score is read half-way between the two years, that is as 6·5. In cases where the same median score is given for three successive years, or for an uneven number of years, as in the Dearborn Formboard second method, for years 10, 11 and 12, the middle year is used, that is, 11 in the example cited. The same value is repeated several times at the higher years in some tests, i.e., at ages above which these tests do not differentiate. A subject whose chronological age is above these ages, is given credit for a mental age equivalent to his chronological age. Thus a 15-year old child would receive a mental age of 15 in the Porteus test if he made the maximum score. The results from the English standardization and from the American were not in all cases found to be entirely comparable,[1] especially in the Adaptation Board, and in the Profile test. Hence medians for the last three years are not given for the Profile test. And, in a few cases, values have been interpolated in the table of norms. The median scores, whether English or American, have in all cases been given to the nearest whole number.

When mental ages for all the tests given have been found from Table A, the median[2] of these mental ages is taken. An intelligence quotient, or "mental ratio," is then computed by dividing the mental age by the chronological in the customary way. The results from the group of tests may be recorded in some such way as that indicated in the accompanying Record Sheet.

Calculating intelligence quotients is, of course, not the only use that can be made of scores from performance tests. It is sometimes desired to compare groups of subjects by means of a given test, as, for example, 9-year boys with 9-year girls in the Porteus test; or to compare results from a given test within a single group, as, for example, the range in the Cube Imitation test covered by 11-year boys. For either of these purposes it seems advisable to leave the results in terms of scores rather than to translate them into mental ages by means of Table A. Hence, in order to be available for comparison, the results in Table B have been left as scores. The terms in which these scores are expressed vary from test to test as already indicated.

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[1] There are two possible explanations for this. In the first place, the English standardization is based on results from practically equal numbers of boys and girls, while in the American standardization, in at least two tests—the Cube Construction and the Dearborn Formboard—the results are almost entirely from men in the U.S. Army. Secondly, the English subjects tested were of a somewhat lower social class than the American, and it may be, with performance tests, as with other tests of intelligence, that individuals from a good environment have an advantage over those from a poor.

[2] In order to find the median all the mental ages are arranged in order from largest to smallest. The median is then that value above and below which there are an equal number of mental ages.

Record Sheet for Performance Tests.

Name

Median Mental Age =
I.Q. =

Tests.	Scores.	Final Scores	Mental Ages.
	Data for scoring.		
1. Porteus ..	Mazes. Cross out incorrect; half marks at side. 5 6 7 8 9 10 11 12 14		
2. Cube Imitation	Lines. Cross out incorrect. 1 2 3 4 5 6 7 8 9 10 11 12		
3. Adaptation Bd.	Points. Cross out those not attained. 1 2 3 4 5		
4. Manikin ..	Points. Cross out those not attained. 1 2 3 4 5		
5. G. Fmbd. ..	1st ; 2nd ; 3rd		
6. Diagonal ..	Time		
7. Substitution	Time ; errors		
8. Triangle ..	Time		
9. P.C.I ..			
10. Cube Construction	Moves Time Model 1 Model 2 Model 3		
11. D. Fmbd. ..	Moves Time A B C D		
12. Healy A ..	Time		
13. P.C. II ..	Pictures 1 2 3 4 5 6 7 8 9 10		
14. Profile ..	Time		

REMARKS—

The chief use in this scale of performance tests of a table for translating scores into mental ages, is to get a median mental age from which an intelligence quotient can be determined, to express the results in one figure when the series of tests is considered as a whole.

TABLE B.

Sex Differences in Performance Tests.

Chronological Age.[1]	Porteus Maze	Cube Imitation.	Adaptation Board.	Manikin Test.	Goddard Formboard.[2]	Diagonal.	Substitution.	Triangle.	Picture Completion I.	Cube Construction.	Dearborn Formboard.[2]	Healy Construction A.	Picture Completion II.	Profile.
8. Boys		7·	4·	4·5	23·	54·	170·	59·	441·	9·	10·3	106·	31·3	241·
Girls		7·	3·5	4·2	25·	194·	157·5	109·	207·5	7·	9·5	132·	22·3	241·
Average	9·1	7·	3·7	4·3	24·	124·	163·7	84·	324·2	8·	9·9	119·	26·8	241·
9. Boys	—	7·	4·	5·2	19·5	54·	162·	69·5	529·5	6·5	14	70·5	29·	175·
Girls	—	7·5	5·	5·2	22·5	37·	115·5	76	454·5	6·	12·5	132·	37·1	189·
Average	9·8	7·2	4·5	5·2	21·	45·5	138·7	72·7	492·	6.2	13·2	101·2	33·	182·
10. Boys	—	7·	4·	4·5	19·	49·	112·	36·	569·	10·	12·3	62·5	53·5	148·
Girls	—	8·	4·5	5·	21·	60·5	109·	94·	454·	8·	11·	92·5	47·5	171·5
Average	10·6	7·5	4·2	4·7	20·	51·7	110·5	65·	511·5	9·	11·7	77·5	50·5	159·7
11. Boys	—	8·	5·	5·	17·	34·	103·	43·5	583·	11·	12·	62·	62·	110·
Girls		7·5	5·	5·	18·	59·	98·	71·	505·	9·	10·	53·5	54·	121·5
Average	11·5	7·7	5·	5·	17·5	46·5	100·5	57·2	544·	10·	11·	57·7	58·	115·7
12. Boys	—	7	5·	5·	16·	45·	109·	23·	580·	12·5	11·	42·5	58·5	70·5
Girls	—	8·	5·	5	16·	39·	82·5	44·	566·	9·	12·	35·	63·	123·
Average	12·3	7·5	5·	5·	16·	42·	95·7	33·5	573·	10·7	11·5	38·7	60·7	96·7
13. Boys	—	7·5	5·	5·	12·	27·5	99·	27·5	608·	11·	15·	41·	66·2	79·5
Girls	—	8·	5·	5·	12·	45·	97·	52·5	566·5	12·	15·5	50·	59·2	72·5
Average	13·2	7·7	5·	5·	12·	36·2	98·	40·	587·2	11·5	15·2	45·5	62·7	76·

[1] The figures in this table for ages 10 and above are based on records from 100 children at each age; except for the Dearborn Formboard at age 13, where 55 children only were tested, and the Porteus test, in which 100 children were tested at all the ages given in the table. Twenty children only chosen as average were tested at ages 8 and 9 with all the tests except the Porteus. All the records given in the table are from pupils in elementary schools under the L.C.C.

[2] Both of these tests were scored by the "2nd method," except at age 13 where records were scored by "1st method."

Attention should be drawn to the marked sex differences shown in Table B. It may be that the differences between boys and girls in performance tests simply illustrate two of the sex differences in various tests of intelligence found by Burt, Terman and others—first, that while girls excel in a linguistic type of test, boys excel in a more perceptual sensori-motor type of test; and second, that girls usually have been found to excel in memory tests, boys in reasoning.

2. *Relation of Results with Performance Tests to Temperamental and other Factors.*

The method of working which a subject employs is often as important as the amount which he does. For example, two individuals may have the same intelligence quotient after being tested with this scale of performance tests. Their reactions, however, may have been very different. One, perhaps, has worked carefully and deliberately, passing all the tests at about the level of his chronological age, and exhibiting no peculiar reactions. The other, however, may have worked hastily and impulsively, doing well in some tests and poorly in others, making scores which range over a number of years, and in general exhibiting erratic reactions. These two methods of work, as well as other methods, may be said to depend on both intellectual and temperamental peculiarities. Whatever the nature of the factors, however, they should be taken into account in considering the results from performance tests. In the scale of tests here described, only one test, the Porteus Maze Test, could be said to measure temperamental factors directly. However, performance tests, even more than most other intelligence tests, furnish valuable opportunities for observing temperamental qualities indirectly. The material used is to most children more novel than Binet's material: and the novelty may have its own special appeal, or perhaps in some at first elicit suggestive signs of timidity. Since so many performance tests are scored on speed, as well as on accuracy, or on speed of reaction alone, it follows that the subject's method of working is more dependent in these tests than in the Binet scale on ability to work under pressure, and to withstand distraction by unessentials in the material or by noises in the room.

It is advisable, in giving this scale, as in other measurements of intelligence, to make careful observations on each subject's performance. The examiner can best learn from experience what to note. However, the following suggestions for observations in giving the various tests may be of some help :—

Cube Imitation Test.—Note whether the subject is *almost* correct in the trials in which he is unsuccessful, i.e., whether he fails by only one mistake, or whether he seems totally unable to grasp the pattern to be reproduced, and simply taps the blocks at random.

Adaptation Board, Manikin.—These two tests are so simple as to require no special notes generally. With very young children or with defectives, however, it is useful to observe whether the subject tends merely to play with the pieces of the Manikin or whether he attempts to make something with it. If the subject is unsuccessful with the Adaptation Board, it is well to note whether failure is obviously due to inattention while the changes in the position of the board are being made.

Goddard Formboard.—Note especially if the subject seems to become excited when hurrying. Note also whether he tries some shapes—especially the star and cross—in any other than the right openings.

Profile.—If the subject fails in this test, it is useful to note the cause of his failure. To be unsuccessful in fitting together the parts of the ear is obviously a much less serious mistake than to fail to see that all the pieces make a head. When the subject has finished, it is permissible, of course, to ask him what the pieces make.

Healy Construction Test A, Triangle Test, Diagonal Test, Dearborn Formboard.— In these tests note if the subject tries the same unsuccessful combination of blocks over and over again. Also observe if he seems to depend entirely on trial and error methods, or on chance.

Picture Completion Tests I and II.—Observe whether the subject tries pieces which are absurd in the various openings. If he leaves these pieces as his final placings, it is sometimes enlightening to ask him to explain his choices, since they are occasionally not so absurd as they seem ; for example, the pumpkin in place of the football (*see* footnote 2, page 21). In Picture Completion II. it sometimes throws light on a subject's method of work to ask him, at the end of the test, to put the pieces back in the box in their proper order. Some subjects do this at once in the quickest and easiest way ; others remove all sixty of the small pieces from the box, and take a disproportionately long time over this simple task.

Porteus Maze Test.—Note whether the subject repeats the same mistake in successive trials with a given maze. Observe especially whether he deliberates before starting and seems to work with a plan, or whether he works impulsively without looking ahead.

Substitution.—Observe if the subject makes a definite plan of procedure, as, for example, filling in all the triangles, then all the circles, and so forth. Although, according to the directions, he is not allowed to follow this plan, it is perhaps an evidence of ingenuity to attempt to do so.

Cube Construction Test.—Note whether the subject is penalized principally for time or for errors in this test. Such an observation may indicate whether he is simply slow in doing the right thing, or whether he makes a number of false starts, and fails to see what is the right thing to do. For example, in the latter case, he may for some time fail to see that the blocks must be piled on top of each other to make a two-inch cube like Model 3.

C.—Comparison of the Tests with other Estimates of Intelligence, and with Tests of Mechanical Ability and "Constructive Ability."

In Table C scores in performance tests are compared with other estimates of intelligence[1], and with results in Stenquist's tests of mechanical ability[2], and Kelly's test of "constructive ability."[3] The results given in this table were all obtained with one hundred 13-year old children in elementary schools.

Table C.

Comparison of Performance I.Q.'s
with
1. *Other Estimates of Intelligence.*
2. *Tests of Mechanical Ability.*
3. *Tests of Constructive Ability.*

	Estimates of Intelligence.				Tests of Mechanical and Constructive Ability.			
	Performance I.Q.	Binet I Q.	Non-Language Group Test.	Teachers' Estimates	Stenquist I.	Stenquist III.	Puzzle-box.	Kelly Test.
Average Score—								
Boys	·95	·94	107·	—	54·7	83·4	4′ 26″	96·8
Girls	·93	·97	—	—	—	70·8	—	72·7
Correlation with Performance I.Q.'s								
Boys—								
Fourteen Tests	—	·41	·51*	·38	·29*	·31	— ·05*	·06
†Six Tests ..	—	·55	—	—	—	—	—	—
Girls—								
Fourteen Tests	—	·49	—	·43	—	·16	—	·21
‡Six Tests ..	—	·67	—	—	—	—	—	—
Correlation with Binet I.Q.'s								
Boys—								
Fourteen Tests	·41	—	·40*	·74	—	—	—	—
Six Tests ..	·55	—	—	—	—	—	—	—
Girls—								
Fourteen Tests	·49	—	—	·73	—	—	—	—
Six Tests ..	·67	—	—	—	—	—	—	—

* 30 subjects. In all other cases there are 52 boys and 48 girls.

† The six tests for boys were · Substitution, Cube Construction, Picture Completion II, Goddard Formboard, Diagonal, and Porteus Mazes.

‡ The six tests for girls were Substitution, Cube Construction, Cube Imitation, Picture Completion I, Picture Completion II, and Profile.

The fact that the correlations between performance tests and the Binet scale, and between performance tests and teacher's

[1] The intelligence tests here referred to are the Stanford revision of the Binet scale (*see* Terman, L. M. The Measurement of Intelligence), and Pintner's Non-language Group Test (*see* Pintner, R. A Non-Language Group Intelligence Test. *J. of App. Psy.*, Sept., 1919). In addition to these measurements, the teachers of the 13-year old children, estimated each child's intelligence and ranked the group.

[2] Stenquist, J. L. Measurements of Mechanical Ability. Bureau of References, Research and Statistics. Board of Education, New York.

[3] Kelly, T. L. A Constructive Ability Test. *J. of Ed. Psy.*, Jan., 1916.

estimates[1], are no higher, can be explained on the grounds that the two types of tests probably measure different aspects or manifestations of intelligence. Comparatively low correlations were after all to be expected; they are in keeping with the original assumption that verbal and non-verbal tests taken together give a more comprehensive view of a subject's mentality than either alone.

Success in Stenquist's tests can be said to depend largely on "mechanical ability," probably a specific capacity in part distinct from intelligence; Kelly's test of "constructive ability" is apparently dependent on originality and perhaps creative imagination; while success in performance tests depends on general intelligence. The correlations between these three types of non-verbal measurements are comparatively low. This result emphasizes the fact that three such different capacities or abilities should not be confused merely because they all make use of a non-verbal method of expression. Performance tests, as above mentioned, should be considered as essentially measurements of intelligence.

I desire to express particularly my thanks to Dr. Cyril Burt, Psychologist in the Education Department of the London County Council, for his untiring assistance throughout the preparation of this work. I should like also to record my gratitude to the Education Committee of the London County Council for their permission to carry out the experiments in their schools, and to the various headmasters and headmistresses who so kindly allowed me to work in their departments. I am also indebted to Miss Lettice Baker, who assisted me in giving some of the tests, and to the National Institute of Industrial Psychology for lending copies of the test materials.

[1] Teacher's estimates, as well as the Binet scale, in view of a number of researches, can be considered as having a marked linguistic bias.

APPENDIX.

Bibliography.

The following list of titles probably includes the most important books and articles concerning the lines of development of performance tests, their standardization and uses.

In some cases, however, these tests are mentioned but incidentally, and it would be misleading to suppose that the articles here referred to describe either such numerous or such extensive experimental studies of performance tests as are to be found, for example, in the literature of the Binet test.

This list is confined to English and American publications. But very little work on tests of this type has, as a matter of fact, been undertaken hitherto in other countries.

The references are classified under three headings, according as they give information about—

(1) The procedure used in performance tests. Articles are included here which have to do with the standardization of tests or scales, and which furnish intensive studies of a specific type of performance test, or of the application of performance tests under specific conditions, as in testing the deaf.

(2) The theory underlying performance tests.

(3) Comparative studies in which performance tests are used. There are included under this head articles which record differences between the sexes and between races, which compare results from performance tests with those from other tests, with diagnoses of mental diseases, with industrial and social ratings, and with estimates of personality traits.

The number or numbers in brackets at the end of each reference indicate which of these three aspects is emphasized principally by the author of the book or article.

The following is a key to the abbreviations for various periodicals mentioned in the bibliography.

Arch. of Psy.—Archives of Psychology.
B. J. Psy.—British Journal of Psychology.
Ed. Psy. Mon.—Educational Psychology Monographs.
J. of Abn. and Soc. Psy.—Journal of Abnormal and Social Psychology.
J. of App. Psy.—Journal of Applied Psychology.
J. of Delin.—Journal of Delinquency.
J. of Ed. Psy.—Journal of Educational Psychology.
J. of Expt. Psy.—Journal of Experimental Psychology.
J. of Expt. Pedagogy.—Journal of Experimental Pedagogy.
Ped. Seminary.—Pedagogical Seminary.
Psy. Bull.—Psychological Bulletin.
Psy. Clinic.—Psychological Clinic.
Psy. Mon.—Psychological Monographs.
Psy. Rev.—Psychological Review.

BASSETT, D. M., and PORTEUS, S. D. (—): Sex Differences in Porteus Maze Test Performance. *Research Publication No.* **32,** *Training School, Vineland, N.J., U.S.A.* [3.]

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INDUSTRIAL FATIGUE RESEARCH BOARD.

Studies in Repetitive Work with Special Reference to Rest-Pauses

By

S. WYATT, M.Sc., M.Ed.

assisted by

J. A. FRASER, M.A., B.Ed.

LONDON:
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PREFACE.

During the last two years the Industrial Fatigue Research Board have paid special attention to the effects of breaking up long spells of light repetitive work by short rest-pauses, and in a report published last year presented the results of their first investigation on the subject.[1] This consisted first of a studied comparison of output data in certain factories as between the period before and the period after the introduction of the rest-pause system, and secondly of laboratory experiments in work devised to reproduce different types of common industrial tasks. The results of both parts of this investigation were positive, and although in the laboratory experiments the beneficial effects of the rest-pause were more pronounced, there were clear indications that under factory conditions also the judicious introduction of rest-pauses may not only tend to reduce monotony and so increase the contentment of the workers, but may also often bring about an increase in output amounting to from 5 to 10 per cent. in spite of the diminution of working time.

This conclusion was based mainly on the study of past records, and, although it covered a large number of workers, it was open to the possible objection inherent in all investigations of this type, namely the difficulty of ensuring that other working conditions were identical over the periods compared and the consequent possibility that the improvement noted was due at least partly to other factors. The Board therefore decided to continue the investigation in the hope that a more definite pronouncement on the effects of rest-pauses might be made, and were fortunate in obtaining facilities in certain factories for the current observations of workers throughout the working period, whereby the necessary allowances could be made for the operation of other variables.

The present Report, which is based on observation of sixteen workers in four factories over periods of fifteen weeks, appears to have justified this hope. Although the workers studied were few in number, the intensive method of investigation adopted has enabled corrections to be introduced for the effects of other factors, and the consistency with which in every instance an increase in the rate of working has followed the introduction of the system, suggests that similar results may always be expected in operations of the kind studied. Further, although this increase is not large, it more than suffices as a rule to compensate for the time actually lost in resting.

In addition to rest-pauses, other points of practical interest are mentioned in the report ; in particular, the marked improvement resulting in certain instances from modifying the conditions

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[1] Two contributions to the study of Rest-Pauses in Industry. *Report No. 25.*

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of work (p. 29), and the possibilities arising from the unconscious similarity in output trend of neighbouring operatives (p. 27).

In presenting this report, the Board think it desirable to review the evidence now available, which strongly suggests that the experimental introduction of rest-pauses, at least for processes involving light repetitive work, will have a successful issue.

Numerous experiments under the controlled conditions of the laboratory with work involving mental rather than physical effort have usually shown that the subject responds to the rest in such a way that his output increases, often by a large amount. This fact in itself is no more than suggestive, for expectations obtained from the laboratory are admittedly often not fulfilled when translated into the factory. In this instance, however, there is no reasonable doubt that in a working spell of 4 to 4½ hours the beneficial effects of a short rest-pause emerge through the many other factors present in industrial work, and cause a genuine increase in output as well as greater comfort and satisfaction for the workers. Lastly, it appears that employers who have had the initiative and foresight to adopt the practice (which significantly enough is far commoner in the United States than in this country) have seldom discontinued it after trial, and it may therefore probably be legitimately inferred that it has proved almost universally successful. It constitutes, indeed, one more instance of how the right application of physiological and psychological principles to industrial practice may benefit both employer and workman, and with the large amount of evidence in favour of the system already available the Board find it difficult to believe that the numerous industries concerned with light repetitive work can long remain indifferent to this obvious and comparatively simple method of reducing the cost of production and at the same time advancing the workers' interests.

A few words of caution, however, are perhaps desirable. First, as was pointed out in the previous report referred to, response to a system of rest-pauses may often not be immediate and a period of some months may elapse before the full beneficial effect is apparent; it follows then that employers should not be discouraged by an absence of any immediate difference, but should persevere with the experiment for at least three months.

Secondly, for the ensuring of success it is important to take into account the position, duration and even the nature of the rest-pause. Some tentative suggestions on these points are made in the present report, but probably experimental adjustment until the best results have been obtained is at present the shortest method to adopt. For this purpose it may be desirable to make use of the services of a special investigator familiar with the subject, such as those now available through the National Institute of Industrial Psychology.

October, 1925.

Studies in Repetitive Work with Special Reference to Rest-Pauses.

By S. Wyatt, M.Sc., M.Ed. *Investigator to the Board*
(assisted by J. A. Fraser, M.A., B.Ed.).

CONTENTS.

1. Introduction.

In recent years the question of rest periods for industrial workers has been receiving an increasing amount of attention. In the early part of the nineteenth century, long hours and unbroken spells of work were considered necessary for profitable production. Thus Senior,[1] in 1837, asserted that profit depended upon the last hours of work, and would be destroyed if the eleventh, or even the twelfth, hour of work were curtailed. Gradually, however, the fallacy of this doctrine became evident, and the hours of work were reduced in successive stages with an accompanying improvement in the quantity and quality of output.

During the recent European War, the necessity for increased production caused a reversion to the longer hours of work, but it was again found[2] that this was a misguided policy, and that the output in a 65 hour week was often no greater than the output in a week of 55 hours. Subsequent investigations have enabled more evidence to be collected on the relation between work and rest—a subject which embraces a very wide field, since it deals with the duration and distribution of the hours of work, the effect of the week-end break, and holidays. The contents of this report, however, are limited to the consideration of rest-pauses introduced within the spell of work.

Under existing industrial conditions, the length of the work-spell is usually from four to five hours. It is frequently asserted that an unbroken spell of this duration is detrimental to efficiency, because of its adverse effects upon the worker. Existing evidence in this connection is somewhat meagre, and is usually based upon the weekly records of wages or output kept by the firm before and after the introduction of the pause. Such records fail to show the more detailed effects of variations in the conditions of work, which are often extremely interesting and suggestive. The investigations about to be described represent an attempt to apply detailed methods of observation to various forms of repetition work in which rest-pauses have been introduced.

2. Description of Processes Investigated.

Facilities for observing women actually at work, and for the experimental introduction of rest-pauses were obtained at three factories. A short description of the processes investigated is given below.

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[1] See, for instance, "Fatigue and Efficiency," by Josephine Goldmark, N.Y., 1912, and "Economics of Fatigue and Unrest," by Florence, p. 69.

[2] Report on "Industrial Fatigue and Efficiency," issued by the Health of Munition Workers Committee. Cd. 9065.

(a) *Handkerchief Folding.*

In this investigation observations were made on eight girls engaged in the process of folding handkerchiefs in the so-called oblong style. They were all experienced workers and varied in age from 18 to 21 years. When working, the girls stood facing each other along the two sides of a long table, the handkerchiefs to be folded being placed on the left and the finished work on the right of each girl.

Each worker received 25 dozen handkerchiefs at a time, which, when folded, were placed on an adjoining table. She then went to the forewoman for a fresh supply, and continued to work in this manner throughout the day.

The hours of work were from 7.45 to 12.15 in the morning spell, with a rest of 10 minutes at 10 a.m., and 1.15 to 5.30 in the afternoon spell. The operatives were paid on a piece-rate basis, but a premium of $16\frac{2}{3}$ per cent. was given on earnings between 15*s*. and 30*s*., and a premium of 25 per cent. on earnings above 30*s*.

In this investigation, observations were limited to the afternoon spell of work, as the manager wished to try the effect of a rest in this spell.

(b) *Hand-Ironing.*

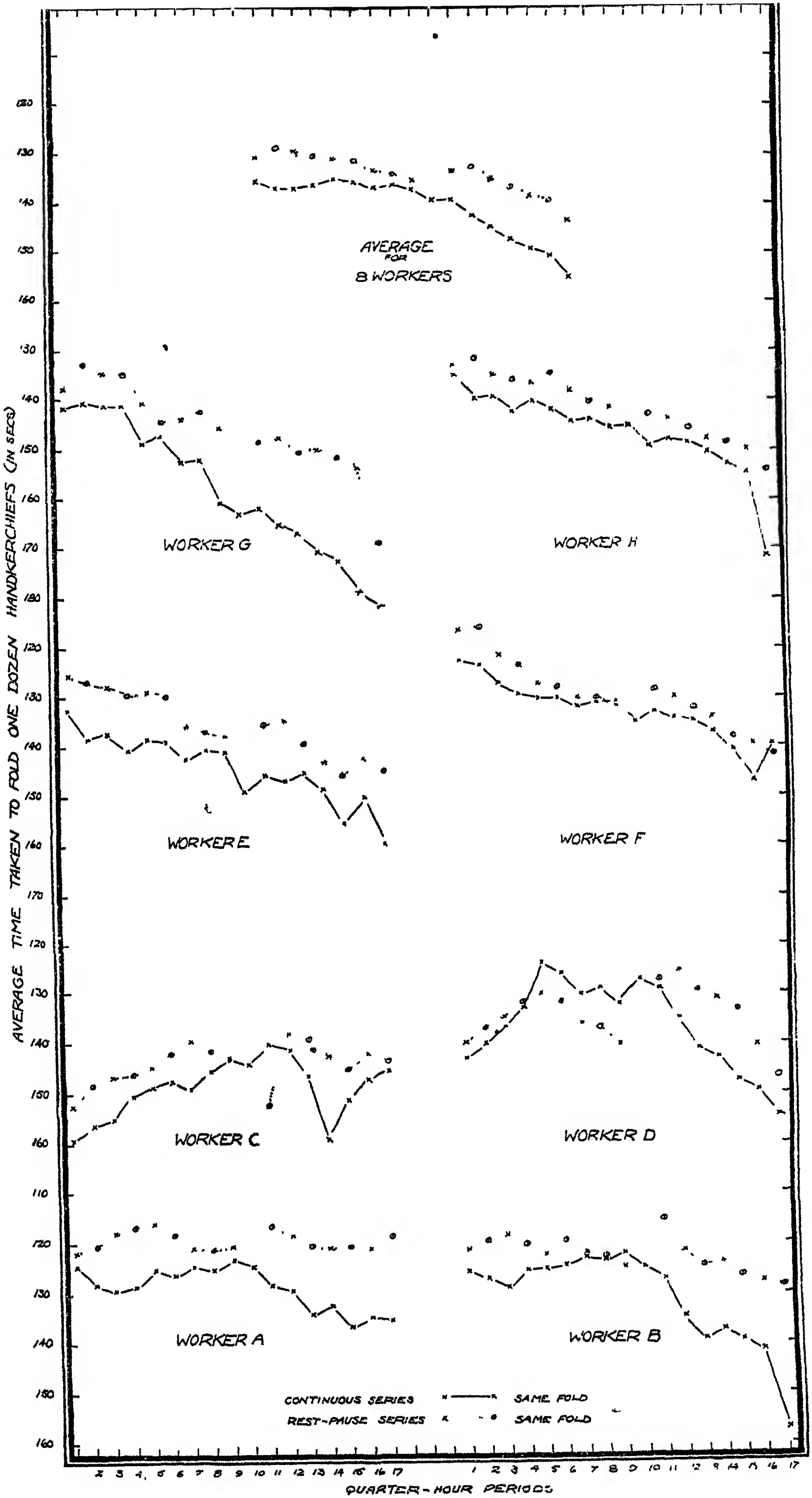
During the course of the investigation on handkerchief folding, some observations were made on hand-ironers employed in the same room. Results were obtained before and after the introduction of the rest-pause, and relate to four experienced workers over a period of four weeks. A gas-heated iron was used by the workers in this operation, and the articles ironed were the handkerchiefs received from the folders. The workers stood at a table while ironing and the task appeared to require a certain amount of muscular strength and dexterity. The hours of work and other conditions were the same as in handkerchief folding.

(c) *Stamping Presses.*

The results presented in this section deal with four women workers engaged in the process of stamping-out cigarette tin lids. Each worker was seated in front of a mechanical press. On her left was a supply of tin sheets from each of which two rows of lids could be punched. The worker took up a sheet, placed one corner of it over a die in the press, and then pressed a lever with her right foot. This movement operated the stamping and cutting mechanism in the press; the lid was cut from the sheet, and removed mechanically to a box. The adjoining part of the sheet was now placed in position on the die, and the cycle of movements again repeated. In this manner 12 lids were punched from each strip, and the remnants of the sheet were placed in a box on the right of the worker. The process accordingly involved a considerable amount of manipulative dexterity, spatial judgment, and muscular endurance.

Table I.—*Comparison of Average Time in Seconds taken to Fold 12 Handkerchiefs during each quarter-hour of the spell in the Continuous and Rest-Pause Series. (Average taken over three weeks in each series.)*

	Worker	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	Avg.
Continuous Series.	A	124·6	128·0	129·5	129·1	125·5	126·5	124·8	125·1	123·5	124·7	128·0	131·8	136·1	134·3	137·0	134·7	134·9	129·3
	B	125·6	127·2	128·7	125·5	125·1	124·5	122·2	123·1	122·1	124·9	127·5	134·3	139·5	137·9	139·7	141·7	157·7	131·0
	C	159·3	156·7	155·1	150·8	149·2	147·4	149·2	145·7	143·1	144·1	140·2	141·4	145·8	159·8	151·8	147·9	145·6	149·1
	D	143·2	140·3	137·5	133·8	124·2	126·7	130·9	129·6	132·8	127·8	129·5	135·8	141·1	143·0	147·9	150·5	154·6	137·0
	E	133·1	138·5	137·2	140·9	138·4	139·2	142·4	140·6	141·0	149·3	146·3	147·1	145·7	148·7	156·1	150·2	159·9	144·4
	F	123·0	123·7	127·4	129·8	130·2	130·2	132·1	131·2	131·4	135·3	133·3	134·1	134·5	137·6	140·8	147·0	139·8	133·0
	G	141·4	140·6	141·7	141·6	149·1	147·3	152·3	152·0	160·3	163·2	162·0	165·4	166·9	170·6	172·8	178·9	181·8	158·1
	H	134·7	139·8	139·1	142·2	140·2	142·1	144·8	144·0	145·6	145·7	149·2	147·3	148·3	150·2	152·9	154·7	171·5	146·6
	Avg. ..	135·6	136·9	137·0	136·7	135·2	135·5	137·3	136·4	137·5	139·4	139·5	142·2	144·9	147·8	149·9	150·7	155·7	141·1
Rest-Pause Series.	A	122·1	120·3	118·0	117·2	116·3	118·4	121·2	121·5	120·7	—	117·2	118·8	120·3	121·1	120·3	120·9	118·6	119·6
	B	121·6	119·9	118·3	120·2	122·5	119·3	121·6	122·6	124·6	—	115·7	122·0	124·2	124·0	126·4	127·5	128·3	122·4
	C	152·7	148·3	146·8	146·3	144·8	142·1	139·9	142·0	143·0	—	152·5	138·5	139·4	142·8	142·5	142·5	146·0	144·6
	D	140·1	137·8	135·7	132·7	131·9	132·5	136·9	137·2	140·0	—	127·9	126·1	129·7	131·4	133·4	140·2	146·9	135·0
	E	126·0	126·8	128·1	129·9	129·0	130·0	135·8	136·9	138·2	—	135·5	135·0	139·8	143·7	146·0	142·5	145·0	135·5
	F	116·7	116·2	122·0	123·8	127·5	128·0	130·3	130·2	132·0	—	128·5	130·0	132·2	134·5	138·2	139·5	141·8	129·5
	G	137·5	132·8	135·0	135·2	140·8	144·9	144·0	142·2	145·9	—	148·5	147·8	150·5	150·0	151·2	153·6	168·9	145·6
	H	132·8	131·7	134·9	135·9	136·2	134·3	138·1	140·6	141·5	—	143·1	143·9	145·9	148·0	148·2	150·1	154·0	141·2
	Avg. ..	131·2	129·2	129·9	130·2	131·1	131·2	133·5	134·2	135·8	—	133·6	132·8	135·3	136·9	138·6	139·6	143·7	134·2



AVERAGE FOR 8 WORKERS
WORKER G
WORKER H
WORKER E
WORKER F
WORKER C
WORKER D
WORKER A
WORKER B
AVERAGE TIME TAKEN TO FOLD ONE DOZEN HANDKERCHIEFS (IN SECS)
CONTINUOUS SERIES — SAME FOLD
REST-PAUSE SERIES — SAME FOLD
QUARTER-HOUR PERIODS

All the workers observed had been employed on this process for several years, and were fully experienced. Their ages seemed to lie between 20 and 30 years, and they were paid on a piece-rate basis. The hours of work were from 8 a.m. to 1 p.m. and from 2 p.m. to 6 p.m., with the exception of Friday, when work ceased at 5 p.m. On Saturdays the operatives worked from 8 a.m. till noon.

3. Effects of Rest-Pauses.

(a) *On Output.*

Handkerchief folding.—Each operative was first observed for three weeks when continuously employed on the same kind of fold throughout the spell. She was then observed for a similar period in which a rest of 10 minutes was given from 3.30 to 3.40. In this process, the worker first completed a dozen handkerchiefs which she placed immediately in front of her, before transferring them to the pile of finished work on her right. The time taken to fold a dozen was accordingly chosen as the unit for statistical purposes, and this method of procedure enabled four operatives to be observed at the same time. The results obtained in each of these two experimental series are given in Table I and Fig. 1.

Hand-Ironing.—In order to compare the rate of working before and after the introduction of the pause, the time taken to iron a dozen handkerchiefs (oblong style) was noted as frequently as possible throughout each afternoon spell. These times were occasionally prolonged because the operatives found it necessary to damp the handkerchiefs before ironing. The results obtained were therefore corrected so as to show the rate of working when the results affected by damping are excluded (Table II).

Table II.—*Average Time (in seconds) taken to Iron 12 Handkerchiefs during each half-hour of the spell in the Continuous and Rest-Pause Series. (Average taken over two weeks in each series.)*

Worker.	1	2	3	4	5	6	7	8	Avg.
				Continuous Series.					
A	87·7	86·7	81·1	83·9	84·5	82·3	84·9	84·6	84·5
B	82·0	82·0	79·7	81·3	78·4	80·3	77·6	79·9	80·2
C	80·6	85·3	87·7	81·8	80·8	77·8	77·3	79·6	81·4
D	87·7	89·6	84·3	87·4	84·9	86·5	86·4	82·5	86·2
Avg. ..	84·5	85·9	83·2	83·6	82·1	81·7	81·5	81·6	83·0
				Rest-pause Series.					
A	79·0	81·3	81·1	81·7	82·0	79·2	81·3	77·6	80·4
B	78·2	77·4	73·7	74·1	72·9	72·1	73·4	73·2	74·4
C	80·9	79·3	76·8	73·0	72·5	74·1	75·1	74·5	75·8
D	83·3	83·3	82·7	81·3	80·8	87·0	85·7	78·8	82·9
Avg. ..	80·3	80·3	78·6	77·5	77·0	78·1	78·9	76·0	78·3

During the first 10 minutes of the spell, the workers were waiting for the irons to become heated, consequently observations on the rate of working were begun at 1.30.

In the rest-pause series, a pause of 10 minutes was given from 3.30 to 3.40, hence the results in the fifth period are based upon observations made from 3.40 to 4.0.

The above results are also represented in Fig. 2.

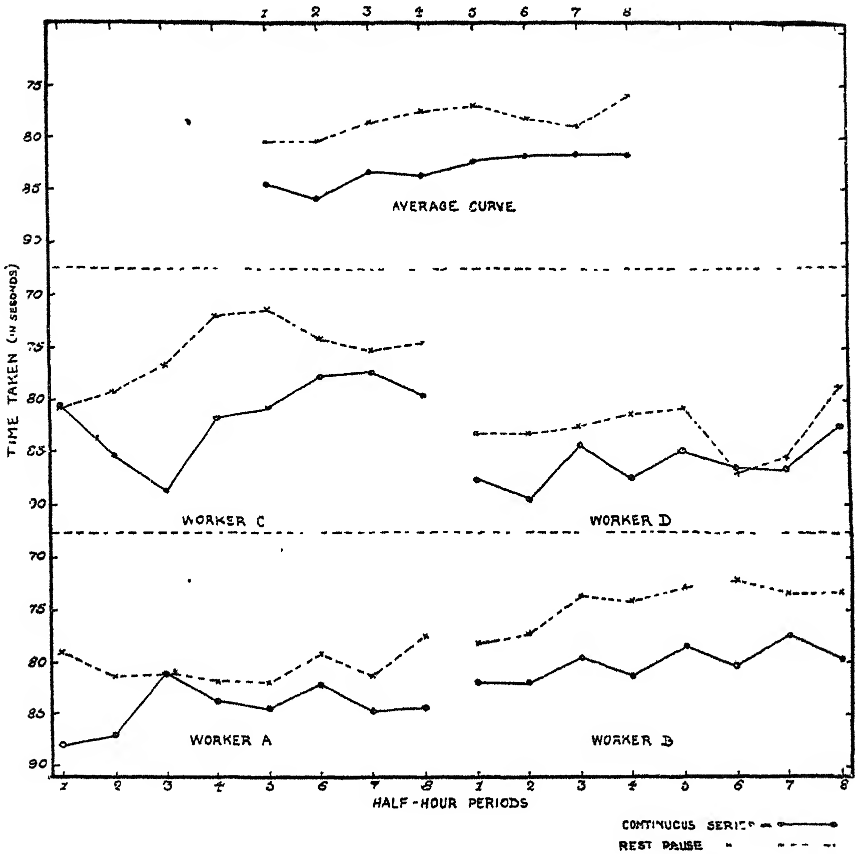


FIGURE 2.—*Comparison of Average Times in Handkerchief-Ironing in the Continuous and Rest-Pause Series. (The time scale reads downwards so as to represent rate of working.)*

Stamping Presses.—In this experimental series, pauses of 10 minutes were introduced at 11 a.m. and 4 p.m., and observations were made under these conditions for a period of three weeks. The results obtained have been compared with those recorded in the preceding continuous series of equal length.

In this investigation the number of sheets completed by each worker was noted every half-hour, and the averages so obtained are given in Table III. The average duration of stoppages (in minutes) for each half-hour is also given, and from these values the nett rate of working has been calculated.

Table III.—*Showing Average Number of Strips Completed, Duration of Stoppages in minutes, and Rate of Working calculated from the output and actual time worked in each half-hour of the spell. (Continuous and Rest-Pause Series.)*[1]

CONTINUOUS SERIES.

Period.	1	2	3	4	5	6	7	8	9	Avg.
Morning Spell.										
Output	66·5	77·5	73·7	81·8	78·2	78·7	79·7	85·8	82·5	78·3
Stoppages	6·2	3·5	5·3	4·3	4·7	4·4	4·5	2·5	2·7	4·2
Nett Rate of working.	83·5	87·7	89·4	95·5	92·8	92·2	93·9	93·6	90·7	91·0
Afternoon Spell.										
Output	75·9	79·4	77·0	81·7	78·1	79·3	78·4	—	—	78·5
Stoppages	3·9	4·6	4·8	3·7	5·0	4·4	4·0	—	—	4·3
Nett Rate of working.	90·7	93·8	91·7	93·2	93·6	92·9	90·5	—	—	91·6

REST-PAUSE SERIES.

Period.	1	2	3	4	5	6	7	8	9	Avg.
Morning Spell.										
Output	76·9	78·1	83·4	84·7	77·7	73·1	92·0	88·6	82·6	81·9
Stoppages	3·5	3·6	2·9	4·7	6·1	6·8	2·7	3·6	4·0	4·2
Nett Rate of working.	87·1	88·8	92·3	100·4	97·6	94·6	101·1	100·7	95·3	95·2
Afternoon Spell.										
Output	80·2	82·4	80·7	75·9	82·4	82·3	80·0	—	—	80·6
Stoppages	4·1	3·8	4·2	4·8	4·1	4·1	4·4	—	—	4·2
Nett Rate of working.	92·9	94·4	93·9	90·4	95·5	95·4	93·8	—	—	93·7

[1] The number of days available for comparative purposes were 7 for operatives A and B and 8 for operatives C and D.

The results obtained in the last period of the morning and afternoon spells have been omitted from the above table as the time worked in each period was only 10 or 15 minutes.

To save space, the results obtained from each worker have not been given separately in the above Table. They are, however, similar in form and consistency to those presented in Tables I and II.

Since the number and duration of enforced rests varied on different days in the two experimental series, it was necessary to select days from the rest-pause series on which the distribution and duration of stoppages were similar to those of the previous series. This procedure, of course, reduces the number of days available for comparative purposes, but increases the accuracy of the results. The results are presented in Table III and Fig. 3.

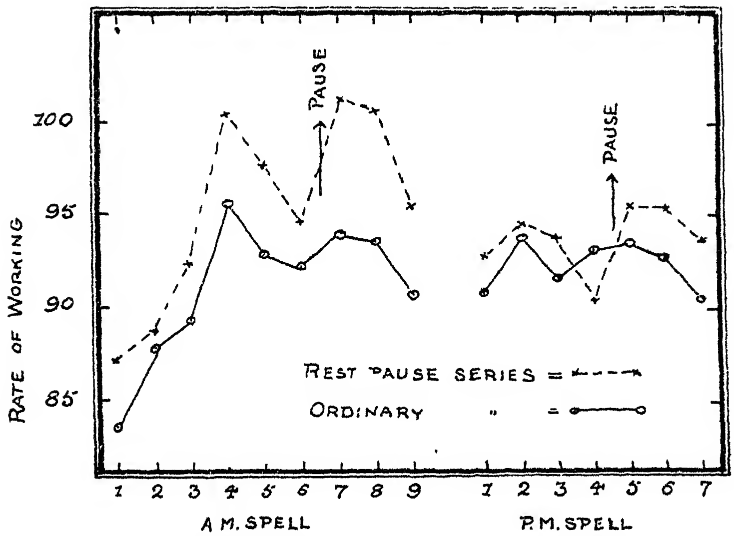


FIGURE 3.—*Average Nett Rate of Working in the Stamping-Press Operation in each half-hour of the spell.* (*Continuous and Rest-Pause Series*)

A comparison of the results obtained in the three processes shows that the introduction of the rest-pause causes in each case a distinct increase in the rate of working. In handkerchief folding the average for all the workers showed an increase of about 5 per cent. and varied from 6·5 per cent. in the case of worker E to 2·7 per cent. for worker F. In handkerchief ironing the increase in the rest-pause series amounted to 5·1, 7·8, 7·4 and 4·0 per cent. respectively for the four operatives, whilst in stamping presses the introduction of two authorised and expected rests caused an appreciable increase in the rate of working during the morning spell, and had a less effect in the afternoon spell (4·6 and 2·3 per cent. respectively). These results are very consistent and are strikingly confirmatory of those already obtained by previous investigators.

In an American inquiry,[1] for instance, it was found that, with one exception, the introduction of rest-pauses was always accompanied by an increased hourly output. Somewhat similar

[1] "Rest Periods for Industrial Workers." Research Report No. 13 of the National Industrial Conference Board.

results were obtained in two large American metal-working establishments.[1] In this country, also, Vernon[2] and Bedford found that the general effects of rests was to cause an increase in hourly output, and his results are supported by the investigations of Farmer[3] and May Smith.[4]

The increase in hourly output produced by the introduction of rests has been effected by an actual increase in the rate of working, due to the stimulating and recuperative effects of the rest, and also by a reduction in the number and length of spontaneous rests. Farmer,[5] also, in 1921, published an account of an experiment with a worker engaged in metal polishing. She was first observed for three days under normal conditions, when she took rests as required, and then for another three days, when organised rests of 5 minutes in each hour were allowed. In the second period, the average time required to complete an article was reduced by 3·6 per cent., and she worked more consistently throughout the day. In 1922, May Smith[6] gave an account of a special study of a hand-ironer in a laundry. The average time taken to iron one shirt was noted in a period without rests, and afterwards when a 15 minutes rest was given at 10 a.m. The results obtained showed a distinct reduction in the time taken during the period with rests.

Laboratory investigation has also shown that the increase in output obtained when rests are introduced is partly due to an increase in the rate of working.[7] In these experiments voluntary pauses were not allowed in the period of work without rests.

Rest-pauses are obviously most satisfactory when they not only increase the hourly rate of production, but also have a favourable effect on total output. In the present investigation, the rates of working observed are equivalent to an increase in total output of 2·3 per cent. in handkerchief folding, 1·6 per cent. in handkerchief ironing, and 0·7 per cent. in the morning spell of the stamping process. In the latter operation, however, the output in the afternoon spell is decreased by 2·7 per cent.

The advantageous effects of a rest in the stamping process were considerably reduced because of the occurrence of numerous enforced stoppages, of a mechanical nature. These amounted, on the average, to a total of 88·2 minutes in a day of nine hours. Such stoppages were very discouraging to the workers, and the prospects of a further decrease in working-time due to the authorised rests failed to evoke the enthusiasm noticeable in the other processes investigated. Rest-pauses are most beneficial when introduced into spells of fairly continuous activity; when

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[1] American Public Health Bulletin No. 106.
[2] Report No. 25 of the Industrial Fatigue Research Board.
[3] Report No. 15 of the Industrial Fatigue Research Board.
[4] Report No. 22 of the Industrial Fatigue Research Board.
[5] Ibid., p. 54.
[6] Report No. 22 of the Industrial Fatigue Research Board, pp. 19–22.
[7] See, for instance, Appendix I, p. 34 of this Report.

added to numerous existing stoppages their effects are less satisfactory. Even in this process, however, the benefits were distinctly positive in the long morning spell, but the increased rate of working due to the rest in the shorter afternoon spell was insufficient to overcome the amount of productive time lost by the rest.

Of 89 American employers reported to have tried regular rest periods, in only one instance was it stated that they had led to a decrease in total output.[1] Farmer[2] also, as a result of the introduction of rest-pauses of 7 minutes' duration about the middle of the morning and afternoon spells of work, obtained an increase in output of 5·47 per cent., despite a reduction in working-time of nearly 3 per cent. Similar results have been obtained by Vernon,[3] who found that the general effect of rests was to induce a slight but genuine improvement in output in almost all the instances investigated, amounting in three cases to 10 per cent. In only one case was there a definite reduction in output (about 3 per cent.).

Suitable rests not only tend to increase output, but have also a favourable effect upon the quality of work. Since, however, the quantitative effects are much easier to measure, the effect upon quality has seldom been determined, but is, nevertheless, important. In the American inquiry,[4] it was found that in a printing establishment the work done in the last hour of the day was of little value. After the introduction of a 10 minutes' rest period in the forenoon and afternoon, a marked increase in accuracy was observed.

Incidentally, it may be noted that in the handkerchief ironing investigation, the effects of damping were less marked towards the end of the spell in the continuous series. Since the necessity for damping was equally distributed throughout the spell, it would appear that the operatives were less particular about the quality of the work as the end of the spell approached. Since in the rest-pause series this effect did not occur, the rest appeared to have a favourable effect upon the quality as well as on the quantity of work done.

In future investigations on the effects of rests, the question of quality should be considered as well as the effect on quantity.

A second characteristic common to all these investigations is the fact that the increase in output is not wholly an after-effect of the pause, but is also noticeable in the period prior to the rest. In handkerchief folding the percentage increase after the pause is 6·8, and is exactly half that amount in the period preceding the rest. Similarly, in handkerchief ironing, the average increase in the rate of working in the periods preceding and following the rest is 6·8 and 5·2 per cent., and in stamping presses, 3·7 and 6·8 per cent.

[1] " Rest Periods for Industrial Workers," op. cit. pp. 19–20.
[2] *J. Nat. Inst. Indust. Psy.* Vol. 1, No. 3
[3] Report No. 25 of the Industrial Fatigue Research Board.
[4] " Rest Periods for Industrial Workers, " op. cit. p. 24.

The most probable explanation of these features of the results is threefold. In the first place, the introduction of the rest stimulated the operatives to greater activity because they thought that the loss of time due to the rest might otherwise result in a smaller total output and a consequent decrease in the amount earned. The effect of this conscious element was particularly reflected in the higher rate of working at the beginning of the spell, followed by the more leisurely rate as the time for rest approached. This incentive seemed to continue throughout the spell, but its effect decreased as the operatives realised that they would be able to equal their former output in spite of the time lost. Statements made by the operatives in response to questions showed that this belief undoubtedly existed and affected their rate of working in the manner described.

Secondly, the rest created a more buoyant attitude towards the work, and enabled the task to be performed with greater interest and ease. The operatives were unanimous in their appreciation of the rest, and often volunteered such remarks as, "It makes the afternoon seem shorter," "The work is not so depressing," and "I feel less tired at the end of work." The decrease in the rate of working observed in the continuous series was partly due to the effects of monotony created by the unvaried activity—effects which were reduced by the anticipation and realisation of the rest.

Thirdly, the rest had a distinctly recuperative effect of a physiological nature. A partial recovery of the fatigued mechanisms took place during the pause, and the greater increase in the rate of working after the rest was largely due to this cause.

The results of other recent investigations have shown that the beneficial effects of a rest are not limited to the period following the rest, but that in many cases favourable results are evident prior to the pause. This feature is particularly noticeable in laboratory experiments,[1] where the work is usually much more intensive than under industrial conditions. The experiments on link assembling, for instance, described on p. 34 show that the increase in output in the period following the rest was 5·3 per cent., while in the period prior to the rest there was an increase of 4·0 per cent.

The above considerations indicate that the expectation of a prolonged period of continuous work impairs the motive forces

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[1] cf. Graf: "Über Lohnendste Arbeitspausen bei geistiger Arbeit." *Psy. Arbeiten.* Vol. VII. No. 4.

May Smith: Report No. 22 of the Industrial Fatigue Research Board. p. 19.

Grunthal: "Über den Einfluss der Willenspannung auf das fortlaufende Addieren." *Psy. Arbeiten.* Vol. VII. No. 3. p. 483.

Thorndike: "The curve of work and the curve of satisfyingness." *J. Applied Psy.* Vol. I. 1917. pp. 265–7.

underlying activity, so that the rate of working is retarded. A prospective rest acts as an additional incentive to activity, and tends to produce an increase in output prior to the rest.

Another subjective effect connected with the introduction of rest-pauses is the time required for complete adaptation to the modified conditions. The introduction of rests may fail to evoke an immediate response, but a gradual process of adaptation usually causes a slow but progressive increase in output.

Vernon and Bedford,[1] for instance, observed that the effects of rests are often not noticeable at once, and in some cases several months are necessary before complete adaptation to the changed conditions takes place. In this respect, the introduction of rests has effects which are analogous to the influence of reduced hours ot work, with the exception that in the latter case the time required for complete adaptation is longer. If, therefore, it is desired to obtain reliable information on the effects of authorised rests, it is necessary to continue the experiment for several weeks, and not be discouraged by the immediate and possibly relatively unfavourable results.

In this connection it may be mentioned that three months after the introduction of the rest-pauses into handkerchief folding, observations were again made for five days on operatives A, B, C, and D. The results showed that the increase in the rate of working, due to the rest, was fully maintained after this lapse of time. (The actual increase was 4·9 per cent., as compared with 4·5 per cent, in the earlier rest-pause series.) The beneficial effects of the rest appear, therefore, to be permanent, as a period of three months is sufficiently long for adaptation to take place. In this latter series, as compared with the earlier observations, there was a slight decrease in the rate of working in the period preceding the pause, but a relatively greater increase when work was renewed after the rest. This difference is presumably due to the decreasing influence of the first factor previously mentioned (p. 12), as the workers become increasingly familiar with the modified conditions of work. The expenditure of less effort in the period prior to the rest will accordingly enable the subsequent recuperative effects of the pause to assume greater prominence.

(b) *Effect of Rest-Pauses on Fluctuations in the Rate of Working.*[2]

Another method of testing the effects of rest-pauses is connected with the fluctuations in the rate of working of the operatives as work proceeds. This test has been carried out in the case of handkerchief folding, when it was found that fluctuations in the time taken to fold consecutive dozens were usually least in the second hour of work and afterwards increased until work ceased. The

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[1] Report No. 25 of the Industrial Fatigue Research Board.

[2] For further evidence of this effect, see Appendix II, p. 38.

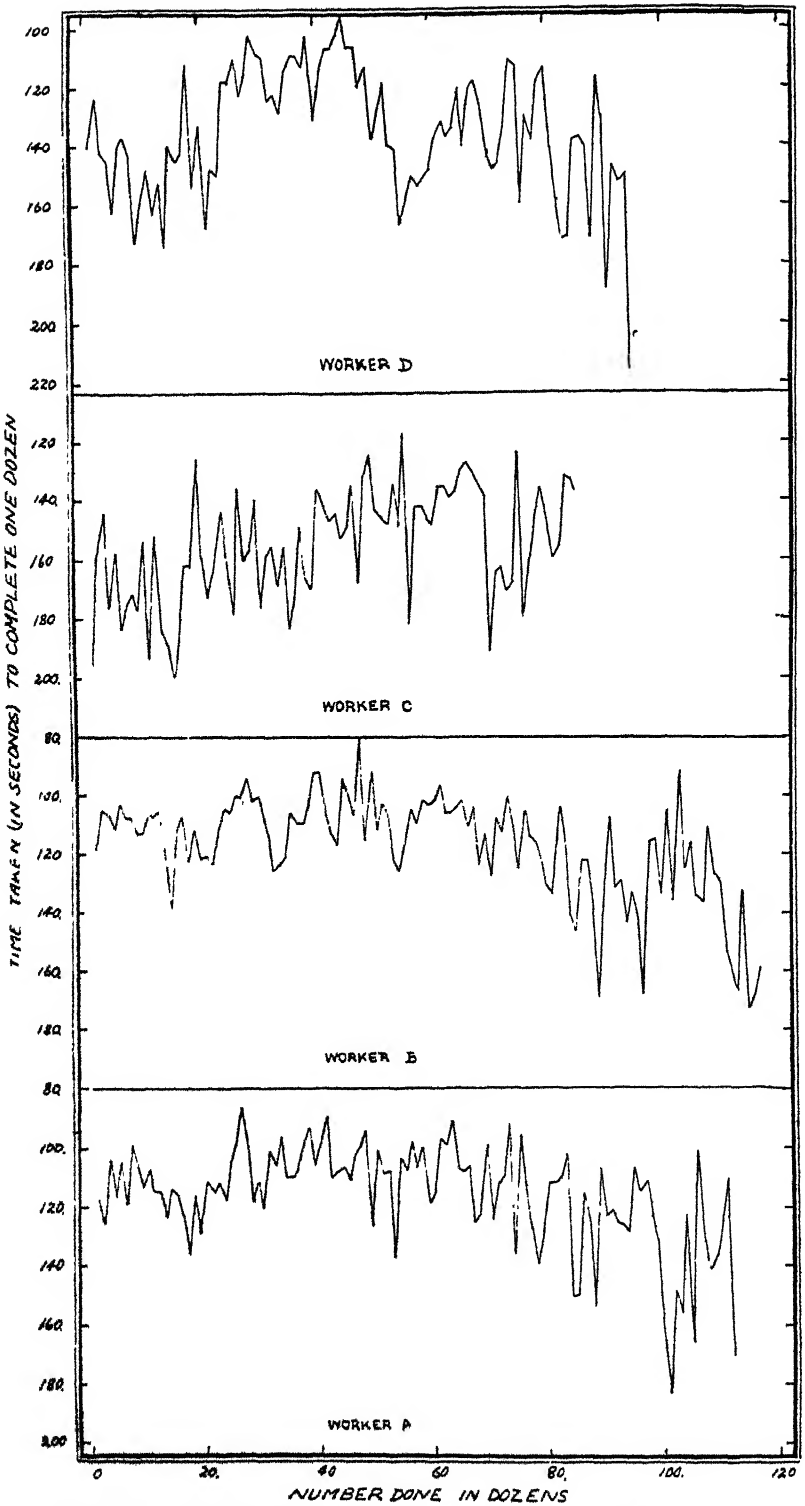

FIGURE 4.—*Variations in time taken to fold consecutive dozens in typical spells of work. (Handkerchief Folding.)*

actual nature and extent of the variations observed are shown in Fig. 4, which represent the results obtained in a fairly typical spell of work.

It is evident that the conditions of work under consideration, which, objectively, remain approximately the same throughout the spell, bring about important variations in the working activity of the operatives. The adverse influences of continued activity under the conditions described are shown not only in the reduced rate of working, but also in increased fluctuations in the time taken to repeat the same cycle of movements as the end of the spell is approached. It is interesting to note that the fluctuations are usually least when the rate of working is at a maximum, and, in general, increase as the rate of working is reduced. In previous investigations of a similar nature, either the rate of working or output has usually been used as a measure of the effect of particular conditions upon working capacity, but it is evident that fluctuations in the rate of working may also be used for the same purpose. In repetitive work of the kind under consideration, the operatives are apparently able to maintain a fairly high and uniform rate of working in the early stages of the spell, when the inhibiting effects of monotony and fatigue are non-existent or comparatively slight. The movements are performed with dexterity and ease, and the task appears to receive almost the whole of the worker's attention. Sooner or later, however, the rate of working begins to decrease and the time taken to complete consecutive cycles becomes more irregular. If the inhibiting processes were confined solely to the muscular mechanisms involved in the performance of the task, we should expect to find a fairly steady decrease in the rate of working as the activity is continued. The increasing irregularity of the results observed, however, suggests that the inhibiting processes have a more central origin, and weaken the attentive powers necessary for the efficient performance of the task. This belief receives support from observations on the behaviour of the operatives, which indicate an increased desire to talk or to seek a change of activity as work proceeds. In future investigations, the effect of conditions of work upon the working capacity of operatives will be shown more clearly and completely if the detailed fluctuations in the rate of working are given in addition to the hourly or half-hourly variations in output.

After the introduction of the rest-pause, it was apparent that the variations in the rate of working throughout the spell were much less than in the continuous series. In order to ascertain the extent of this difference, use was made of the differences in the time taken to fold consecutive dozens throughout the spell. If the average of such differences is obtained for each quarter-hour of the spell, we obtain an indication of the variations in the rate of working at different stages in the spell of work. This method of procedure has accordingly been

adopted, and the results obtained are given in Table IV and Fig. 5. They refer to the activities of workers A, B, C and D, when engaged on the same kind of fold and represent the average of the results obtained from each worker on five days for the continuous and rest-pause series.

Table IV.—*Average Differences (without regard to sign) in time taken to fold consecutive dozens.*

Continuous Series.

¼-hour period ..	1	2	3	4	5	6	7	8	9
Avg. Differences .	19·7	17·8	16·3	15·6	16·8	15·4	14·0	15·1	13·5
¼-hour period ..	10	11	12	14	14	15	16	17	Avg.
Avg. Differences .	12·8	14·3	16·4	18·7	19·0	21·5	19·2	22·2	17·0

Rest-pause Series.

¼-hour period ..	1	2	3	4	5	6	7	8	9
Avg. Differences .	14·0	13·3	14·0	13·1	14·5	13·8	14·3	15·5	18·4
¼-hour period ..	10	11	12	13	14	15	16	17	Avg.
Avg. Differences .	—	17·9	15·3	14·0	13·1	13·7	16·5	23·2	15·3

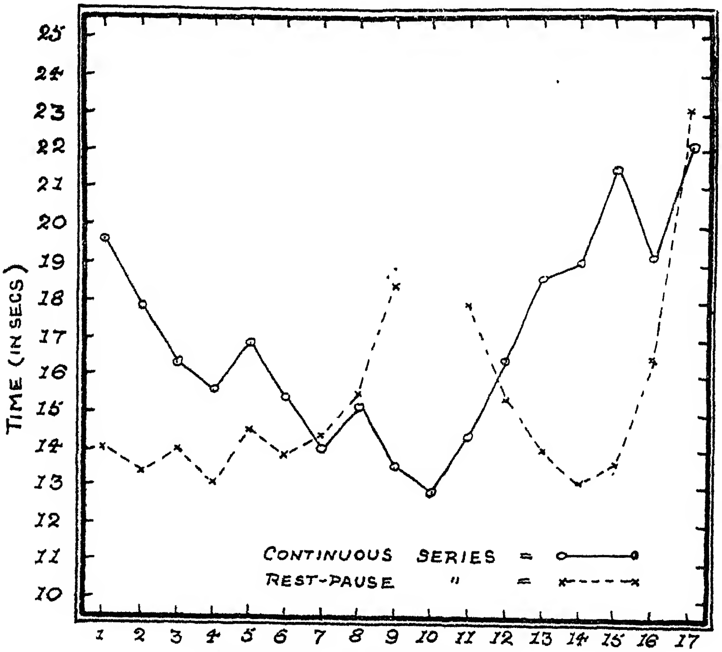

FIGURE 5.—*Average Differences (without regard to sign) in time taken to fold consecutive dozens.*

The results show that the rest has a considerable influence upon the variations in the rate of working at different stages of activity within the spell. The variability in the rest-pause series is not only less than in the continuous series (an average of 15·3 compared with 17·0) but follows a very different course. In the rest-pause series it is least in the early part of the spell,

and increases in a fairly regular manner until the pause. When work is resumed it is still high, but decreases rapidly to a minimum and afterwards rises more rapidly until work ceases. Thus, when the rest is introduced, the workers not only maintain a higher rate of working throughout the spell, but also work more consistently and with greater uniformity. As in the case of the rate of working, a greater consistency is noticeable in the period prior to the rest.

The above results provide additional evidence in support of the belief that the beneficial effects of a rest are not only due to the partial elimination or neutralisation of the fatigue products produced by activity, but are also associated with the creation of a more favourable attitude towards the task.

(c) *Effect of Rest-Pauses on Productive Time.*

The introduction of rest-pauses may also have an appreciable effect on the amount of time lost within the spell of work. In the course of the investigation into handkerchief folding, every stoppage or interruption to activity was carefully noted, and the results obtained are given in Table V.

Table V.—*Average amount of time lost (in seconds) due to different causes in the afternoon spell of 4¼ hours (Continuous Series).*

Worker.	Starting Work.	Stopping Work.	Changing over.	Talking.	Absent.	Miscellaneous.	Total.
A ..	114	179	813	128	175	149	1,558
B ..	142	184	721	82	63	234	1,426
C ..	168	103	780	8	56	103	1,218
D ..	158	135	857	44	90	122	1,406
E ..	173	149	1,044	61	81	252	1,760
F ..	217	166	1,088	18	29	179	1,697
G ..	176	233	1,024	86	44	249	1,812
H ..	211	170	909	22	23	195	1,530
Average ..	170	165	905	56	70	185	1,551

Thus the amount of time lost in the afternoon spell was approximately 26 minutes, that is, the actual folding operation was interrupted to this extent in the spell of 4¼ hours. A little less than 3 minutes was lost in starting work, and about the same time at the end of the spell. By far the greatest amount (15 minutes) was lost in the process of removing finished work and obtaining other handkerchiefs on the completion of every twenty-five dozen. Talking and being away from the bench were responsible for another 2 minutes, and various interruptions, such

as speaking to the forewoman, trouble with the folding board, getting additional handkerchiefs to replace those which were defective, etc., accounted for an additional 3 minutes.

Most of the stoppages are accordingly avoidable, or at least would not occur if the conditions of work were slightly re-arranged. By insisting on promptness in starting and stopping work, and arranging for a more speedy removal and renewal of supplies, the output of each operative for the afternoon could be increased by approximately nine dozen handkerchiefs, or 9 per cent.

The average number of stoppages in the spell of $4\frac{1}{4}$ hours was 15, indicating that the folding process resembles other repetitive handwork processes in being by no means truly continuous.

The effect of talking is not adequately represented in the above table, which gives only the actual stoppages due to talking. As a rule the operatives carried on an intermittent conversation while folding, and it was noticeable that in such cases the folding times were considerably increased. The effect of talking upon the rate of working is shown to some extent in Fig. 6, which is based upon the observations made upon workers E, F, G and H over a period of two weeks. The continuous line represents the actual rate of working observed (including the occasions when talking was in progress), and the broken line gives the rate of working excluding all times affected by talking.

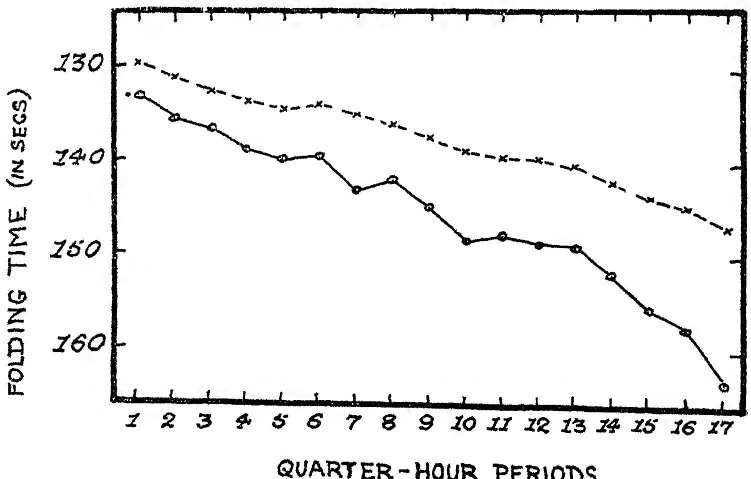

FIGURE 6.—*Comparison of Average Time in seconds taken to fold 12 handkerchiefs in each quarter-hour of the spell with and without conversational accompaniment.*

Thus talking has a considerable effect upon the rate of working, and the effect increases as work proceeds. The working position of the operatives is conducive to conversation, and as the afternoon advances the inclination to talk apparently increases, presumably because of the somewhat monotonous

nature of the task. Though the rate of working might be increased if the facilities for conversations were reduced, the task would be rendered more monotonous, and the operatives would find the folding less congenial.

In order to ascertain the effect of the pause upon the duration of stoppages within the spell of work, the results obtained in the rest-pause series have been subjected to the same method of treatment as those of the continuous series (Table V) and the averages obtained are given in Table VI.

Table VI.—*Average amount of time lost (in seconds) due to different causes in the afternoon spell of 4½ hours (Rest-pause series).*

Worker.	Starting work.	Stopping Work.	Changing Over.	Talking.	Absent.	Miscellaneous.	Total.
A ..	15	88	980	59	163	156	1,461
B ..	15	129	762	41	53	164	1,164
C ..	92	130	756	42	50	94	1,164
D ..	74	147	818	25	64	104	1,232
E ..	100	117	826	61	82	222	1,408
F ..	156	104	939	47	71	192	1,509
G ..	50	139	832	53	69	222	1,365
H ..	105	155	728	86	59	254	1,387
Average ..	76	126	830	52	76	176	1,336

Thus the introduction of the pause has an appreciable effect upon the amount of time lost within the spell of work. The average decrease during the rest-pause series amounts to 14 per cent., the improvement being due almost entirely to a greater promptness in starting and stopping work and changing over.

4. Other Results Observed.

The close observation required for studying the effects of the rest-pauses has elucidated several other points of interest.

(a) *General Characteristics of the Work-Curves.*

Handkerchief folding.—Considering the spells of work when the activities were *continuous*, the results show that, with the exception of worker C, there is a marked decrease in the rate of working during the latter half of the spell. (Fig. 1, p. 5.) In the case of workers E, F, G and H, the rate of working continues to decrease throughout the entire spell; the difference in output between the first and last hour of work for worker G is approximately 25 per cent. The decrease in question illustrates the large variations in working activity which are to be found in repetitive hand-work of the type under consideration.

Handkerchief ironing.—In complete contrast with handkerchief folding, the general tendency for the rate of working in the continuous series is to *increase* throughout the spell. (Fig. 2.)

Are we to infer from this that operatives, manipulating an iron continuously for a period of four hours and exposed during that time to a temperature of approximately 80° F. are entirely unaffected by fatigue or other factors which act adversely upon working capacity ? It seems unreasonable to expect that such immunity is possible, yet the rate of working actually continues to increase as work proceeds.

A partial explanation of this phenomenon is probably to be found in a consideration of the type of work performed. Ironing is essentially light *muscular* work involving fairly regular cycles of activity and comparative rest. The movements are also repeated in a fairly rhythmical manner throughout the spell. It is generally agreed that a suitable rhythmical alternation of light muscular effort and rest will enable the movements to be continued for long periods of time without any diminution in the amount of work done. The beating heart, a slow rate of walking, and ergographic results are examples of such conditions. Hand ironing also appears to be a process of the same type, and its rhythmical nature, together with the distribution of effort and comparative rest involved, enable it to be performed with slightly increasing efficiency throughout the spell.

An alternative assumption would be that the activity is fatiguing, but that the fatigue is reflected in the quality of the work performed ; in other words the time taken to iron a dozen handkerchiefs might remain approximately the same throughout the spell, but the ironing might be done less thoroughly, with fewer movements, as work proceeded and fatigue increased. This possibility receives some support from the fact that less time is spent in damping as the end of the spell approaches. (See page 11).

Stamping presses.—The records of output obtained at corresponding times during each half-hour of the working day (continuous series) have been combined and averaged, and are given, together with the duration of stoppages and rate of working, in Table VII and Fig. 7.

Table VII.—*Showing average number of strips completed, duration of stoppages in minutes, and rate of working calculated from the output and actual time worked in each half-hour of the spell (averages taken over three weeks.)*

Morning Spell.

	1	2	3	4	5	6	7	8	9	10	Avg.
Output	72·4	79·5	80·6	83·8	84·6	82·9	83·5	82·1	67·3	38·3	75·5
Stoppages ..	4·1	3·0	3·2	3·9	2·7	3·0	3·3	3·4	7·7	15·9	5·0
Rate of working..	83·9	88·3	90·2	96·3	93·0	92·1	93·8	92·6	90·5	81·5	90·6

Afternoon Spell.

	1	2	3	4	5	6	7	8	9	10	Avg.
Output	79·4	78·7	76·7	79·9	85·1	84·0	74·0	60·5	—	—	77·3
Stoppages ..	3·6	5·0	5·1	4·3	2·6	3·2	5·3	8·9	—	—	4·8
Rate of working..	90·2	94·4	92·4	93·3	93·2	94·0	89·9	86·0	—	—	92·0

Note.—This Table differs slightly from Table III because it is based upon all the results obtained during the scheduled working time over a period of three weeks. c.f. footnote p. 8.

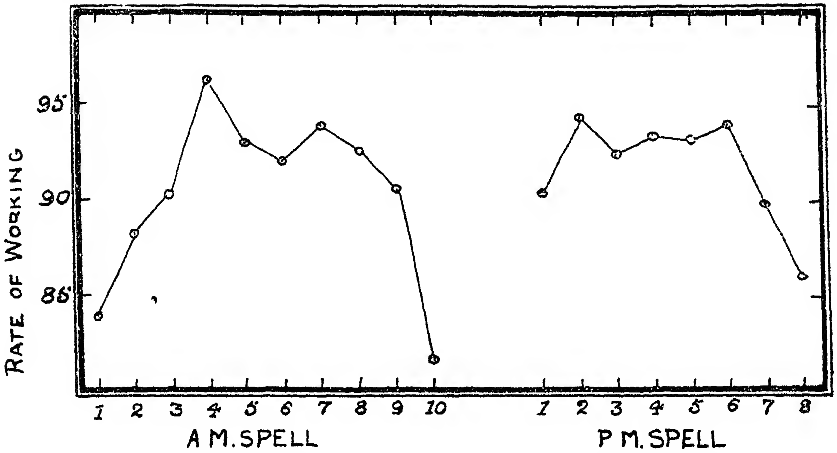

FIGURE 7.—*Average Rate of Working in each half-hour of the spell in the Stamping-Press Operation.*

The rate of working is calculated from the average output and the actual time worked during each half-hour. Friday's results are not included because of the shorter duration of the working day.

The results show that the rate of working is slightly higher in the afternoon than in the morning spell of work, in spite of the fact that the operatives had already been at work for five hours in the morning. It would accordingly appear that the long morning spell does not give rise to any appreciable degree of fatigue. The rate of working during this spell may, however, be adversely affected by the mental attitude created by the prospects of a long day of uninteresting and apparently monotonous activity. Even on the days when the morning stoppages are shorter and less numerous, the afternoon rate of working is still not inferior to that of the morning spell, as Table VIII shows.

Table VIII.—*Average Number of Strips Completed, Duration of Stoppages in minutes, and Rate of Working calculated from the output and actual time worked during each half-hour on days when morning stoppages were shorter and less numerous than usual.*

Output.		Stoppages.		Rate of Working.	
a.m.	p.m.	a.m.	p.m.	a.m.	p.m.
78·3	76·4	3·4	4·5	88·3	89·0

Thus the lower rate of working in the morning spell supports the belief that the conditions of work have a damping effect upon the inclination to work, especially in the early part of the day.

The results also show that, in spite of the numerous and lengthy stoppages, the rate of working in the long morning spell is somewhat variable, and tends to decrease about the middle and towards the end of the spell. In the afternoon spell, the variations in the rate of working are much less. These features of the results give additional support to the views already expressed, and it would appear that the greater efficiency and uniformity observed in the afternoon spell are due to the desire to make up for lost time and the fact that the afternoon spell is shorter and represents the last lap in the day's work.

The rate of working of the operatives is certainly affected by the unusually large number and duration of enforced stoppages, and tends to be higher, especially in the latter part of the spell of work, than would be the case if the activities performed were more continuous. In order to obtain some information on this point, the results obtained on days when the total duration of stoppages was comparatively small have been compared with those on days of longer and more numerous stoppages, and are given in Table IX. When the total duration of stoppages on any day was greater than 80 minutes, it was considered to be a day of many stoppages; when less than 80 minutes, it was taken as a day of fewer stoppages. This division enabled the same number of days (six) for each worker to be included in each class

Table IX.—*Comparison of Average Number of Strips Completed, Duration of Stoppages in minutes, and Rate of Working calculated from the output and actual time worked in each half-hour of the spell on days of many and fewer stoppages. (Stamping Presses.)*

Days of many stoppages :—

Morning Spell.

	1	2	3	4	5	6	7	8	9	10	Avg.
Output	68·6	73·2	74·3	81·5	85·8	85·1	88·0	78·7	67·3	36·8	73·9
Stoppages ..	5·8	5·3	5·5	5·2	2·9	3·3	2·8	4·7	8·7	17·1	6·1
Rate of working..	85·0	88·9	91·0	98·6	95·0	95·6	97·1	93·3	94·8	85·6	92·8

Afternoon Spell.

	1	2	3	4	5	6	7	8	9	10	Avg.
Output	77·8	78·2	74·6	72·3	84·5	81·9	67·4	57·2	—	—	74·3
Stoppages ..	4·5	4·5	5·9	6·8	3·5	4·1	7·9	10·2	—	—	5·9
Rate of working..	91·4	92·0	92·8	93·5	95·7	94·9	91·5	86·7	—	—	92·5

Days of fewer stoppages :—

Morning Spell.

	1	2	3	4	5	6	7	8	9	10	Avg.
Output	73·5	82·4	83·1	84·6	85·3	81·0	79·8	85·7	68·7	40·5	76·5
Stoppages ..	3·4	1·9	2·3	3·1	2·4	2·7	3·7	1·7	6·3	14·6	4·2
Rate of working..	82·9	88·0	90·0	94·4	92·7	89·0	91·0	90·8	87·0	78·9	88·9

Afternoon Spell.

	1	2	3	4	5	6	7	8	9	10	Avg.
Output	81·4	80·2	83·5	87·2	85·9	86·2	80·1	64·5	—	—	81·1
Stoppages ..	2·8	4·2	2·3	2·0	1·9	2·6	2·9	7·4	—	—	3·3
Rate of working..	89·8	93·3	90·4	93·4	91·7	94·4	88·7	85·6	—	—	91·1

The above results are also shown in the following curves :—

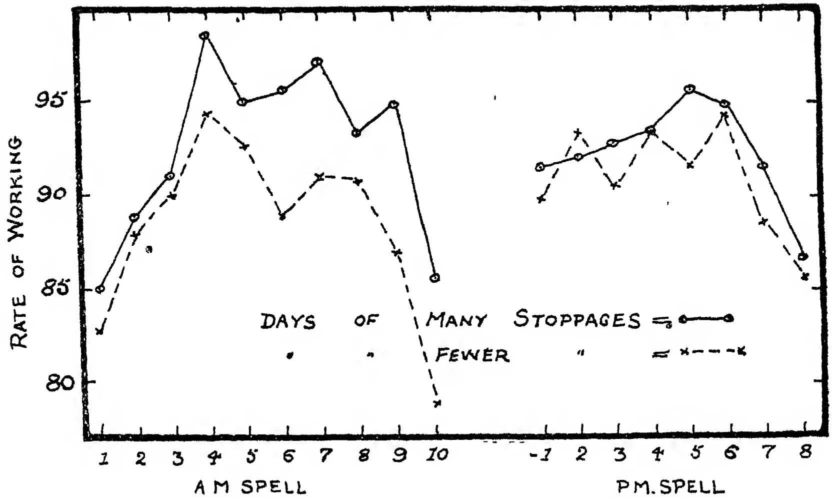

FIGURE 8.—*Rates of Working on days of many and few stoppages*

The results show that on the days when the total duration of stoppages was greater than 80 minutes, the rate of working is higher than on the days of fewer and shorter stoppages. The difference is greater in the morning than in the afternoon spell of work, and is most noticeable in the latter half of the morning spell. An increase in the average hourly duration of stoppages from 8·4 minutes to 12·2 minutes in the morning spell produces an average increase in the rate of working of 4·4 per cent. In the afternoon spell, a corresponding increase from 6·6 minutes to 11·8 minutes gives an average increase in the rate of working of only 1·5 per cent.

In general, therefore, the results show that the rate of working increases as the number and duration of enforced stoppages increase, but the effect is most noticeable in the latter half of the long morning spell.

(b) *Individual Differences.*

Handkerchief Folding.—In addition to the results already given, records were obtained of the time taken by each operative in folding a single handkerchief at different stages in the spell of work, and the averages of these results are given in Table X. They refer to the same kind of fold, and include only the uninterrupted times, consequently they represent the actual rate of working of each operative when free from the disturbing factors mentioned later.

Thus individual differences in the rate of working are very considerable; the greatest difference between any two workers being 42 per cent. (B and C). Since the above results are free

from any observable disturbing factors, the variations must be largely due to innate differences in the abilities required in handkerchief folding.

Table X.—*Individual Differences in the Average Time taken to Fold a single Handkerchief.*

Worker.	A.	B.	C.	D.	E.	F.	G.	H.
Number of observations	87	87	92	99	90	73	92	108
Average time taken (secs.)	6·8	6·4	9·0	7·9	7·8	8·4	9·1	8·4
Standard deviation[1] ..	0·91	0·82	1·42	0·92	1·03	1·09	1·04	0·91

[1] The difference between the means of any two workers is significant in 26 cases out of 28, i.e., the difference between two means is more than three times its probable error.

In addition to the uninterrupted folding times given above, a record was also made of the nature and extent of interfering factors. These have been classified and are given in Table XI.

Table XI.—*Analysis of Factors responsible for an Increase in the Folding Time (Handkerchief Folding).*

Worker.	Stretching and Smoothing.	Re-folding.	Talking.	Unnecessary Movements.	Clumsy Movements.	Total per cent.
A ..	8·5	5·6	5·0	0·9	2·2	22·2
B ..	5·7	2·5	7·5	3·6	0·8	20·1
C ..	0·6	1·3	4·1	5·1	2·1	13·2
D ..	3·1	2·0	1·6	3·1	2·2	12·0
E ..	6·0	5·8	5·9	2·7	0·3	20·7
F ..	2·6	1·3	5·7	2·4	1·2	13·2
G ..	2·4	3·6	8·2	1·9	0·5	16·6
H ..	1·2	6·4	4·3	0·5	0·3	12·7
Average ..	3·7	3·6	5·3	2·5	1·2	16·3

The above results are based upon 150 observations for each operative, and are expressed as percentages of the total folding time. Thus the average amount of time lost because of the interfering effects of the factors given in the Table is 16·3 per cent. Of this total, the necessity for stretching and smoothing some of the handkerchiefs before folding is responsible for 3·7 per cent. A certain amount of stretching is a necessary preliminary to the folding of each handkerchief, but the results given above refer to handkerchiefs requiring special treatment in this respect. Interruptions of this type are accordingly largely avoidable, but as they are due to defective calendering, an attempt might

be made to improve the quality of the work in this preceding process. The other disturbing factors given in Table XI are largely subjective in origin, but they are responsible for a total loss of time amounting to 12·6 per cent.

Differences between the methods employed by the workers were very noticeable, and in some cases many unnecessary movements were made. Worker C, for instance, instead of smoothing the handkerchief with one firm movement after each fold, patted it gently along the creases. A little instruction in the best method of folding would eliminate much of the inefficiency produced by defective or unnecessary movements.

In other cases it was equally evident that the increased times observed were due to inability to carry out the necessary movements with dexterity and speed, owing to a crude sense of touch or to imperfect hand and eye co-ordination. Instruction in such cases would probably be useless since the defect appears to be innate and not susceptible to the effects of training. It appears, therefore, that many of the differences observed are due to innate differences in the abilities required in the folding process, and it is highly probable that applicants who are likely to become expert folders could be discovered by means of suitable psychological tests.

Handkerchief Ironing.—In this operation, some interesting differences were observed in connection with the process of damping. This additional operation is occasional and is rendered necessary whenever a crease occurs in the handkerchief to be ironed. It will be seen that the damping process is responsible for a considerable increase in the ironing time, and because, in addition, it interferes with the rhythmical nature of the work, must be considered an undesirable feature of the ironing process. A detailed analysis of the effect of damping is given in Table XII

Table XII.—*Showing proportion of time spent in damping.* (*Handkerchief Ironing.*)

	A	B	C	D
Ironing time per doz. when damping (in secs.) ..	108·9	92·4	95·1	99·9
Ironing time per doz. when not damping (in secs.).	84·5	80·2	81·4	86·2
Percentage increase in time per doz. due to damping.	28·9	15·2	16·8	15·9
Percentage of total affected by damping ..	45·5	22·5	27·0	23·0

The results relating to operatives B, C, and D, are fairly similar both with regard to the time spent in damping and the number of handkerchiefs damped. Operative A, however, damped twice as frequently as the other workers and spent approximately twice the time in damping each dozen handkerchiefs. Since all the operatives were working with the same kind of material under similar conditions, there appears to be no reason for the unusual behaviour of operative A, and steps should be taken to bring her into line with the other workers.

Imperfections in the handkerchiefs to be ironed, due to faulty work on the part of the folders, were frequently a cause of considerable annoyance to the ironers. In such cases, the difficulties of the ironing process were considerably increased, and the annoyance was expressed in typical gestures and mutual complaints. Operative B, however, who seldom talked while working, preferred to air her grievance to the forewoman.

Operative D talked more than any of the other workers. She usually instigated and maintained all conversations and was generally the first to stop work at night. Almost invariably, she spent the last five or ten minutes in changing her shoes and making preparations for departing. Her rate of working (apart from the time taken in damping) was slower than that of the other workers.

Stamping Presses.—In this process some individual differences were observed in connection with the nature, duration, and distribution of stoppages. An analysis of the stoppages which occurred during the second and third weeks of the experimental series is given in Table XIII. The results refer to an average working day of nine hours.

Table XIII.—*Duration of different kinds of stoppages in minutes.* (*Stamping Presses.*)

Worker.	Starting and Stopping.	Repairs.	Getting material.	Arranging lids.	Counting strips.	Absent.	Resting.	Total.
A ..	18·7	43·9	0·7	8·0	0·0	2·5	7·3	81·1
B ..	19·9	44·0	0·3	7·2	0 0	9·4	4·6	85·4
C ..	20·0	49·2	2·3	5·9	0·0	12·0	8·2	97·6
D ..	19·7	43·7	10·1	7·6	9·9	14·0	6·0	111·0

The results show that almost half the total duration of stoppages was due to mechanical repairs and adjustments, and that a further 20 minutes of the scheduled day was lost at the beginning and end of work. Such stoppages were not only directly responsible for a productive loss equivalent to 45 minutes a day in the case of each worker, but were a cause of annoyance and dissatisfaction to the workers and liable to create an unfavourable attitude towards the work. Other annoying interruptions sometimes occurred when the workers were compelled to fetch the material which should be supplied by a girl appointed for that purpose. Worker D in particular appeared to suffer in this respect. This worker also adopted the unusual procedure of counting the strips as they were received, presumably because of her doubts regarding the number supplied. Time lost in this manner amounted, on the average, to a total of 10 minutes in a day of nine hours. Stoppages of a voluntary nature, which include absence from the room, talking, resting, and walking about for no specific purpose, were responsible for a loss of 16 minutes per day. Workers C and D often used such stoppages as a pretext for a change in activity.

Irregularly recurring and unexpected stoppages usually tend to interfere with the rhythm and swing of work, and frequently occur when they are not wanted and are least needed. They are often annoying interruptions to continued activity, and have a disturbing effect upon the attitude towards the task.

(c) *Comparison of Authorised with Enforced Stoppages.*

The data obtained in the stamping-press investigation may also be used to show the comparative effects of an authorised and expected rest of 10 minutes and enforced stoppages of the same duration. For this purpose, the results obtained during the first 4½ hours of work in the morning spell have been used, and days in the rest-pause series on which the total duration of stoppages (including the 10 minutes' pause) were similar to the total duration of stoppages in the ordinary series have been compared.

The results (Table XIV) are expressed in terms of the duration of stoppages and actual output in a spell of 4½ hours, and the rate of working is calculated from these values.

Table XIV.—*Comparison of effect of enforced but unexpected stoppages with that of authorised and expected stoppages*[1]. (*Stamping Presses.*)

	Output.	Stoppages.	Rate of working.
Ordinary series ..	734	41·2	86·6
Rest-pause series ..	768	41·0	90·6

It appears, therefore, that within the limits described an authorised and expected rest of 10 minutes is superior to enforced and irregular stoppages of the same total duration, the actual increase being 4·6 per cent. The difference may be due to the probability that in the case of enforced and unexpected stoppages, the stimulating effect connected with the prospect of a certain rest is missing. It is also affected by the absence of the beneficial effect of "mass suggestion" in the case of enforced stoppages, since in these cases the operatives are compelled to pause at different times. Such results indicate that a quantitative answer may be found to the assertion that authorised rests are unnecessary when conditions of work involve numerous unforeseen and unavoidable stoppages, and support the belief that a pause is most beneficial when it is expected and occurs at a definite point in the spell of work.

(d) *Effects of Aggregation.*

When operatives engaged on repetitive processes work in close proximity to each other, suggestion and imitation seem to have an important influence upon their activities. A situation is created which apparently influences all the workers in the group,

[1] The data given in Table XIV are based upon the results obtained on four days each of the experimental series.

and may counteract the particular desires or inclinations of an individual member of the group. In handkerchief folding, operatives A and B worked facing each other at the same table, but were some distance from the other operatives. Similarly workers C and D constituted a neighbourly pair. A decrease or increase in the rate of working of one of these operatives appeared to be reflected in the activities of the other worker, as if each were distinctly aware of the behaviour and rate of progress of the other. These features of the results are shown in the following curves, which are fairly typical of the curves obtained at the same time under similar conditions of work.

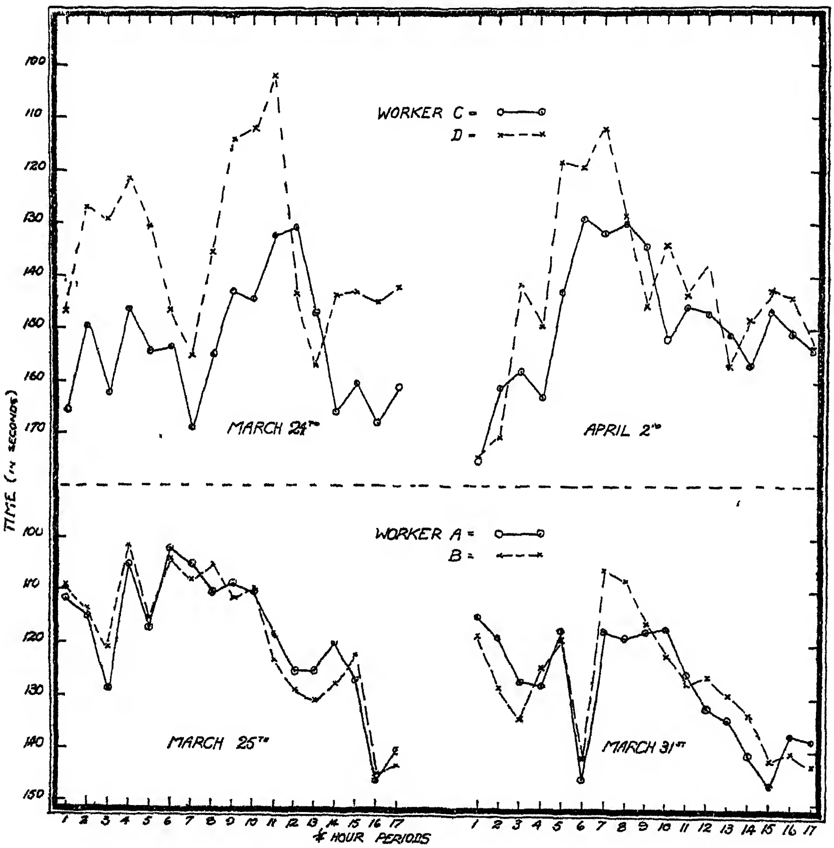

FIGURE 9.—*Showing similarity in rate of working in the case of operatives working in close proximity to each other. (Handkerchief Folding.)*

Operatives E, F, G and H formed a compact group at the same table, and the general shape of the curves obtained from these workers is also very similar (cf., Fig. 1, p. 5.)

When two operatives are working in close association, as in the cases given above, there is some evidence to indicate

that the quicker worker controls the pace, especially when a decrease in the rate of working occurs. In almost every instance in the above curves a decrease in the rate of working in the case of the quicker worker is followed a little later by a decrease in the other member of the pair. From the productive standpoint this is a feature of some importance, since it means that the pace of the slower worker is regulated by the rate of working of the quicker operative, and the former is induced to maintain a rate which she would otherwise probably not attain if placed in close proximity to an operative who was her productive equal or inferior. In other words, the combined efficiency of the two workers is probably higher when working in close proximity than when working in isolation.

In the case of a group of workers, the behaviour of a few industrious members of the group will probably be similarly contagious, and will act as an incentive to greater activity upon the slower workers. It is important, therefore, to include a few reliable and efficient operatives in any compact group of workers, in order that the more indifferent workers, by means of emulation, suggestion, and imitation, may be induced to maintain a higher standard of efficiency.

These statements, of course, are merely tentative and require further experimental evidence before they can be accepted as a general truth. It may be noted, however, that managers in other processes have observed a similar phenomenon, especially when the workers differ only slightly in ability. When individual differences in working activity are large, the slower workers tend to become discouraged or indifferent to the activities of the quicker operatives.

(e) *Effect of Modified Conditions of Work.*

Four months after the introduction of the rest in handkerchief folding, a further modification in the conditions of work was tried. Since an official pause of 10 minutes was allowed about the middle of the spell, it appeared that the large amount of unproductive time absorbed in obtaining supplies of handkerchiefs on the completion of every twenty-five dozen (about 14 minutes per operative per spell) was unnecessary, and arrangements were made to eliminate this loss by engaging a young girl to keep the operatives supplied with raw material and to remove the finished work. Observations on this phase of the investigation were confined to the workers A, B, C, and D, and a period of two weeks under the modified conditions has been compared with an ordinary period of similar length. The results obtained are given in Table XV.

Under the modified conditions of work the time lost between the folding operations was reduced from 669 to 298 seconds (a reduction of 55 per cent.) and that due to interruptions during

the folding process from 804 to 339 seconds (a reduction of 58 per cent.) The actual output under the modified conditions showed an average increase of five dozen, or 4·8 per cent., but this was accompanied by a slight decrease in the average rate of working (0·9 per cent.). Even when the necessity for removing finished work and obtaining raw material was removed, the rate of working was by no means continuous, but was interrupted to the extent of 10·6 minutes in a spell of 4¼ hours, although an additional rest of ten minutes was allowed.

Table XV.—*Effect of modified conditions on Output and Lost Time.* (*Handkerchief Folding.*)

Worker.	Usual conditions.			Modified conditions.			Per cent. increase in output.
	Time lost.	Inter-rupted time.	Output (in dozs.).	Time lost.	Inter-rupted time.	Output (in dozs.).	
A ..	882	859	96	302	255	102	6·3
B ..	567	977	97	278	418	101	4·1
C ..	475	815	90	282	364	94	4·4
D ..	751	565	90	332	321	94	4·4
Average	669	804	93	298	339	98	4·8

" Time lost " represents actual time lost *between* the folding operations.
" Interrupted time " represents time lost *during* the folding operations.

The workers in question were initially somewhat opposed to the new arrangement, presumably because it represented a " tightening-up " of the old conditions and an interference with established habits, but afterwards they became more reconciled to the modification. Since a substantial rest was allowed about the middle of the spell, any advantage due to the change involved in removing and fetching work was rendered less necessary.

Stamping Presses.—In this investigation, the tin strips from which the lids were punched were usually of uniform size, and allowed 12 lids to be punched from each strip. The following diagrams illustrate the method adopted.

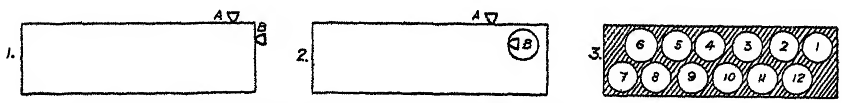

FIGURE 10.—*Method of punching lids from strips.* (*Stamping presses.*)

No. 1 shows the position of the strip with reference to the guage-pegs A and B, ready for the first punch to be made. No. 2 shows the strip in position for the second punch, and No. 3 the

appearance of the finished strip. After stamping out the first row of 6 lids, the strip was turned so that the seventh lid was in the position formerly occupied by the first.

In the course of this investigation, operative D was sometimes supplied with strips of different length. Normally, 12 lids were punched from each strip, but on certain days strips giving 10, 14, and 16 lids were used. Table XVI shows the relative output for the different sizes of strips.

Table XVI.—*Effect of different sizes of strips on output.* (*Stamping presses.*)

Size of strip.	Total output.	Time worked (mins.).	Strips completed per hour.	Lids produced per hour.
10	1,342	420	191·8	1,918·0
12	14,869	4,981	182·4	2,188·8
14	888	319	167·1	2,344·0
14	951	372	153·4	2,454·4

Thus, as the size of the strip increases, the number of strips completed per hour decreases, but the output (lids produced) increases.

The results are, of course, limited to one worker over comparatively short periods of time, but they are very suggestive, and indicate that production would be considerably increased by using the larger size of strips. In the case of the smaller strips, a larger proportion of time is lost in handling the strips between the actual punching operations. Beyond a certain point, large strips become unwieldy and difficult to manipulate, consequently there must exist an intermediate size of strip which is the most suitable from the productive standpoint. This example illustrates a principle of considerable importance in industry, and one which is common to many industrial operations.

Another feature of some importance which emerged during this enquiry is connected with the design of the machines. The stamping-presses in use were similar in type and dimensions, yet the physical proportions of the operatives using them were very different. As a result, the bigger operatives were compelled to work in a very cramped and compressed position, which was decidedly ugly to behold and painful to endure. Such workers often complained of the uncomfortable posture enforced by the design of the machine, and this factor was undoubtedly responsible for many of the stoppages recorded in this work, since the workers found it necessary to obtain relief from the working position. The design of these particular machines is very inadequately adapted to the physical characteristics of the operatives, and is in consequence conducive to much unnecessary discomfort and

reduced efficiency. The machines may be satisfactory from the mechanical and functional standpoint, but their effect upon the operatives has not been sufficiently appreciated. A considerable improvement would be effected if, in future, designers of machines would work in conjunction with the industrial psychologist or physiologist, so that while retaining mechanical perfection, the machines could be adapted to the psychological and physiological requirements of the workers.

5. Summary.

The following summary is given to recall the principal conclusions reached; but the reader should refer to the text and the detailed discussions on which they are based.

1. The introduction of a rest of ten minutes about the middle of the spell of work caused—

(*a*) An increase in the nett rate of working which varied from 1·5 to 8·0 per cent. The increase occurred not only in the period following the rest, but also in the period preceding the rest in each spell.

(*b*) With the exception of the afternoon spell in the stamping-press process, an increase in total output. In handkerchief folding the actual increase was 2·3 per cent.; in handkerchief ironing, 1·6 per cent.; and in the stamping-press process (morning spell), 0·7 per cent. These are smaller than have been found in other instances.

(*c*) A reduced variability in the time taken to repeat the same cycle of movements as the end of the spell approached.

(*d*) A reduction in the amount of time lost within the spell of work.

(*e*) Increased contentment and satisfaction on the part of the operatives.

2. The results obtained in the three processes investigated are very consistent, and suggest that favourable results would be obtained by the introduction of rests in most repetitive processes of a similar type.

3. The work-curves obtained in the different processes differed from each other in many respects, the most desirable form of curve being obtained in handkerchief-ironing. A comparison of the general shape of work-curves obtained in different processes will enable the general effects of particular conditions of work to be ascertained.

4. Individual differences in the methods employed and the rate of working were very noticeable in these investigations. These appeared to be due to innate differences in ability and to

the different methods of instruction. They could be considerably reduced by the institution of a scientific method of selection and greater attention to training.

5. An authorised and expected rest is more favourable than enforced but unexpected stoppages of the same duration.

6. When operatives engaged on repetitive processes work in close proximity to each other, a situation is created which apparently influences all the workers in the group, and may counteract the particular desires or inclinations of an individual member of the group.

When two operatives are working in close association, there is some evidence to indicate that the quicker worker controls the pace, especially when a decrease in the rate of working occurs.

7. In handkerchief folding, a slight modification in the conditions of work caused a reduction in the amount of lost time of over 50 per cent. and an average increase in output of approximately 5 per cent.

8. In the stamping-press operations, it was found that the output depended upon the size of strip used, and that successive increases in the size of strip produced corresponding increases in output.

APPENDIX I.

Laboratory Experiments on Rest-Pauses.

The repetitive process used in these experiments consisted essentially in placing bicycle-chain links on a pair of steel shafts. Three male research workers took part in the experiments, which were begun when they were thoroughly practised in the performance of the task. The work-period was of 2½ hours duration, and the results of three tests in which a pause of 10 minutes was introduced half-way through the spell, have been compared with the results of a similar number of tests in which the work was continuous. The results obtained are represented in Fig. A which give the average output for each five-minute period throughout the test.

The beneficial effect of the pause is clearly indicated in the curves. In every case, the output in the rest-pause series is generally higher than in the interrupted series, particularly in the first 30 or 40 minutes immediately following the rest. A comparison of the output before and after the rest with the output in the corresponding part of the continuous series is given in Table A.

Table A.—*Comparison of Output in Continuous and Rest-Pause Series.*

Subject.	Continuous Series.			Rest-pause Series.			Percentage Increase.		
	1st half.	2nd half.	Average.	1st half.	2nd half.	Average.	1st half.	2nd half.	Average.
A	131·4	127·7	129·5	138·2	137·0	137·6	5·2	7·3	6·2
B	131·2	124·1	127·6	138·2	130·5	134·3	5·3	5·1	5·2
C	138·8	129·3	134·1	141·0	134·0	137·5	1·6	3·6	2·5

In the course of these experiments, it was felt that a rest would be more advantageous if introduced earlier in the spell of work. The desire to pause often occurred after work had been in progress about 45 to 50 minutes, and it was decided to try the effect of introducing two rest-pauses of 5 minutes instead of a single pause of 10 minutes as before. The work-spell was accordingly divided into three equal periods of 50 minutes each. Only subject B was available for this part of the experimental series, and the value of the results will be limited accordingly. The combined results of two tests in the continuous series have been compared with the corresponding results of two tests in the rest-pause series, and are given in Fig. B.

The superiority of the work with rest-pauses over the continuous series is again evident, especially in the middle and latter parts of the spell. The actual output at different stages of the work is given in Table B.

Table B.—*Comparison of Output in Rest-Pause and Continuous Series.*

	1st part.	2nd part.	3rd part.	Average.
Continuous series	140·1	126·9	127·8	131·6
Rest-pause series	147·8	144·1	142·7	144·9
Percentage increase ..	5·5	13·6	11·7	10·1

Thus, in the case of this subject, the increase in output when two rest-pauses of 5 minutes each are given is approximately twice the increase when only one rest of 10 minutes is allowed. As in the previous series, the advantageous effects of the rest are most pronounced about the

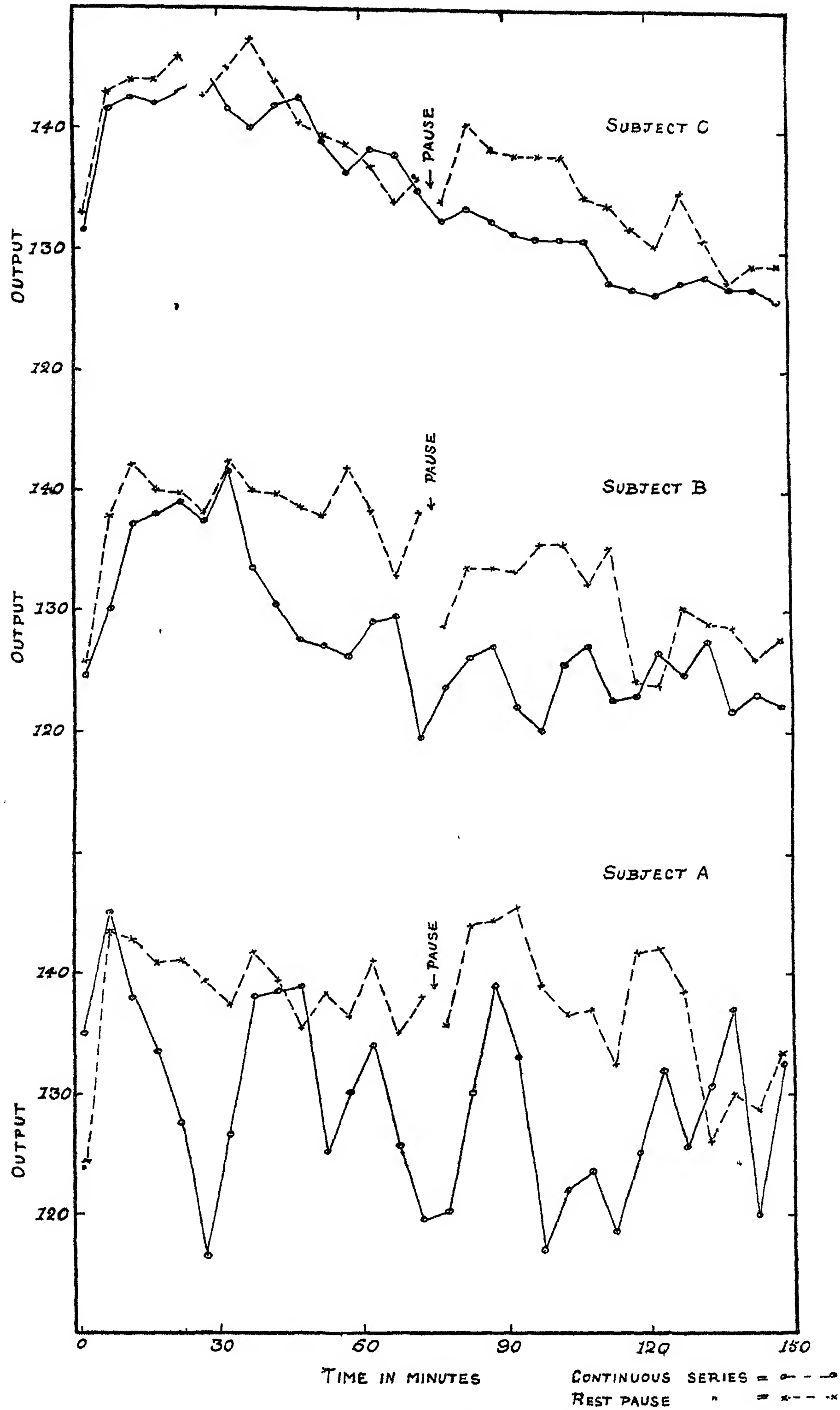

Figure A.—*Effect of rest on output*

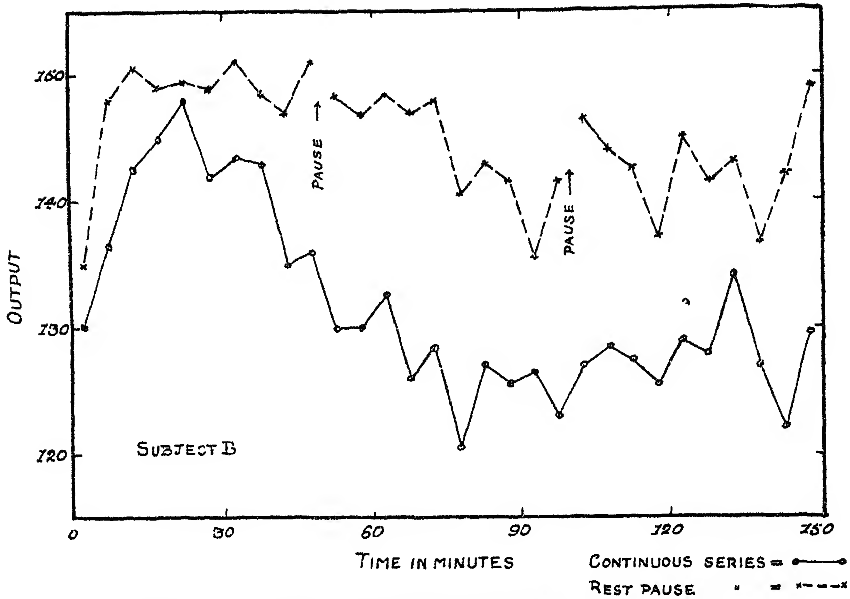

FIGURE B.—*Effect of two rests of five minutes each on output.*

middle portion of the work-spell, due to a diminution of the monotonous effects associated with the continuous series. The anticipation of the pauses caused an increase in output in the first part of the work of 5·5 per cent., which is practically the same as the corresponding increase observed in the single pause series.

The subject expressed a preference for two pauses of 5 minutes rather then one of 10 minutes, and this subjective attitude is reflected in the results. He stated than when the period of work was divided into spells of 50 minutes each, he was able to work throughout at full pressure, whereas in a continuous test he felt that he wanted to distribute his energy over the whole period. The difference in the conditions was compared to the behaviour of a runner in a race of 100 yards and one of a mile.

In order to determine the effect of unorganised rests, three tests were given in which the subjects rested whenever they felt inclined to do so. For purposes of comparison, three tests in which the subjects worked continuously have been used, and the averages of the two series are given in Table C.

Table C.—*Effect of unorganised Rests upon Output.*

	A.	B.
Continuous series..	154·5	145·3
Interrupted series	156·6	152·0
Per cent. increase	1·4	4·6

Thus there is an increase in the average output per unit of time in the rest-pause series in the case of both subjects, although subject B improves more than three times as much as subject A. This is probably because subject B found the continuous series much more monotonous than subject A, and, subjectively, found the rests more beneficial. In each interrupted test, subject A took three rests of 407 seconds average length, but subject B preferred four rests of 152 seconds average length. Thus the total duration of the rests taken by subject A in each test was approximately twice the total length of the rests taken by subject B, and yet the increase in output of the former subject was less than that of the latter.

In every case, the first rest was taken after work had been in progress from 40 to 50 minutes, when the subjects began to feel the monotonous effects of the work and believed their output to be decreasing. The subjects also stated that the last rest taken, although approximately equal in length to the previous rests, did not seem to have the same recuperative effects. The results confirmed this belief, and the fact further illustrates the value of progressively increasing the duration of rests as work proceeds.

The anticipatory effects of rest-pauses is shown by a comparison of the results obtained in work-periods of 2½ hours' and 30 minutes' duration. In this experimental series, six tests were given on consecutive days. On the first, third, and fifth days the length of the work-period was 30 minutes, but on the other three days it was 2½ hours. In each case the subjects were informed of the length of the work-period before the test began.

The average output for each 5 minutes during the first 30 minutes of work in the 2½ hours test has been compared with the output at corresponding times in the shorter test. The results obtained are given in Table D.

Table D.—*Effect of Work-Periods of different length on Output.*

No. of 5-minute period.	Subject A.			Subject B.		
	Long Test.	Short Test.	Percentage Difference.	Long Test.	Short Test.	Percentage Difference.
1 ..	157·3	163·0	+3·6	151·0	149·3	−1·1
2 ..	167·0	171·3	+2·6	149·3	156·0	+4·5
3 ..	162·0	171·0	+5·6	150·3	158·7	+5·6
4 ..	164·0	166·0	+1·2	155·7	159·3	+2·3
5 ..	155·0	165·3	+6·6	153·7	162·7	+5·9
6 ..	156·7	163·7	+4·5	155·0	162·3	+4·7
Average ..	160·3	166·7	+4·0	152·5	158·0	+3·6

With one exception, the output in the short test is always higher than the output at the corresponding time in the long test. The subject's knowledge that he has to endure an unbroken period of 2½ hours intensive work appears to have a depressing effect upon his activity, which, compared with a work-period of 30 minutes, causes a reduction in output of approximately 4 per cent. in the first 30 minutes of work.

From this it appears that, within limits, the shorter the duration of work, the greater is the psychological incentive to work as rapidly as possible. Other things being equal, it appears inadvisable to continue repetitive work uninterruptedly for long periods.

Summary.

The foregoing results give some indication of the subjective and objective effects of different arrangements of rest-pauses introduced into a work-period of 2½ hours. Within the experimental limits described, they show that two pauses of five minutes each are more favourable than a single rest of 10 minutes' duration.

Perhaps the most interesting effect of a rest is the modification it produces in the attitude of the subject towards the task; an attitude which is comparable to the effect induced by a short period of work in comparison with a task of longer duration.

The results further suggest that rest-pauses will be particularly beneficial in repetition work of a monotonous nature, providing that they are introduced in accordance with the indications disclosed by the objective and subjective effects of continuous activity.

Since the length of the work-period under consideration was only 2½ hours, it is desirable to try the effect of longer periods and to repeat the experiments under industrial conditions. The differences between laboratory and industrial conditions may be productive of widely different results.

APPENDIX II.

The Increase of Variability with Fatigue: Note on an Experiment in Arithmetic. By G. Udny Yule, F.R.S.

In Section 3 (b) of the Report it is shown that the time taken to execute a given operation apparently tends to become more variable as fatigue increases. Nearly thirty years ago, in September, 1896, I made an experiment on my own rate of working, in doing arithmetic, which brought out the same result exceedingly clearly.

A number of multiplication sums were prepared, each of 6 digits by 6 digits. In selecting the figures zeros and units were not allowed, and two identical digits were not allowed to be placed together. In working the sums, if there were two identical digits in the multiplier, the rule was made that the multiplication had to be gone through independently each time. I then spent a morning, from 10.15 to 1.15, in working out these sums as fast as I could. The net time taken for each sum was noted with a stop-watch, i.e. the time taken from the actual start of the arithmetic, after the figures had been copied out, to the completion of the product; but if there was any error in the answer that particular sum was simply omitted from the record: the times are those for consecutive sums worked *correctly*. Fifty-three sums in all were done in the time, the total net time being 5078·8 seconds, or about 84½ minutes, so that less than half the gross time was occupied in "productive" work.

The chart, Fig. C, shows the individual times for the successive sums

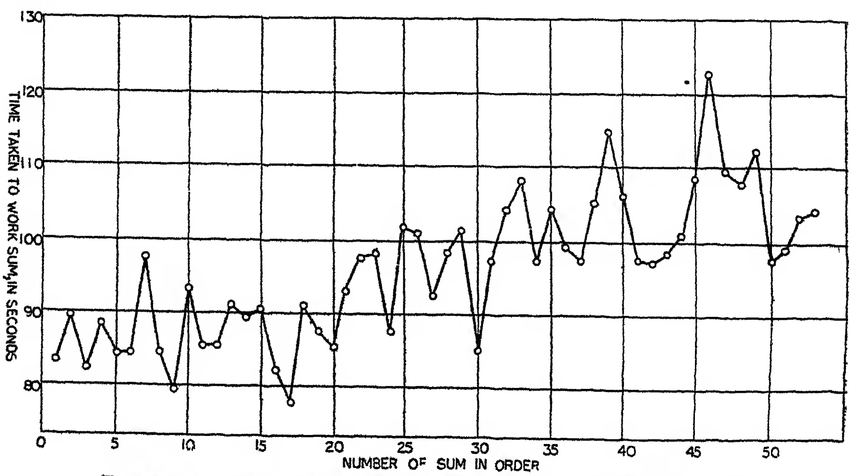

Figure C.—*Times, in seconds, taken to Work fifty-three Successive Multiplication Sums.*

and brings out clearly the increase in the amplitude of fluctuations as the end of the morning was approached. The following table, Table E, gives the mean times and the standard deviations for four (overlapping) groups of twenty sums each. For the first twenty sums the standard deviation is only 4·64 seconds, for the last twenty it is 7·01 seconds. The standard deviations for the two central groups do not run quite regularly, but this is due to the fact that they are appreciably affected by the increasing times. The correlation between the time taken for each sum in the group, and its number in the group, is given in the next column, and hence we obtain the corrected standard deviations of the last column, obtained by the formula $\sigma\sqrt{1-r^2}$; and hence giving the standard deviation about the best-fitting straight line through the segment of the chart representing the twenty sums in question. These corrected standard deviations increase continuously from the top to the bottom.

Table E.—*Means and Standard Deviations for groups of* 20 *sums in the series of* 53 ; *r is the correlation between the time taken by the sum and its number in the group.*

Sums.	M.	σ	r	$\sigma\sqrt{1-r^2}$
1–20	86·70	4·64	·024	4·64
11–30	91·22	6·66	·553	6·08
21–40	99·13	6·59	·453	5·88
34–53	103·86	7·01	·254	6·78

If the corrected standard deviations of the last column are multiplied by 100 and divided by the means, we get the co-efficients of variation—5·35, 6·08, 5·93, 6·53 : although the figures do not run quite regularly, there is a marked increase in relative, as well as absolute, variability between the first group and the last. Apart from the increase of variability as measured by the standard deviation or co-efficient of variation, the chart suggests a much more marked tendency to work in spurts or bursts towards the end of the period, as if the curve were becoming definitely oscillatory. But, of course, these results are purely individual, and different individuals are likely to vary largely.

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PREFACE.

The Board in their Preface to a previous Report have already stated the reasons which induced them to devote part of their resources to research on methods of vocational guidance and selection. In accordance with this policy they decided as the first step to arrange for the compilation of a review of the existing literature on the subject.[1] Subsequently two other Reports were issued, the one comprising the results of three investigations on vocational selection in certain restricted fields of work,[2] and the other containing descriptions of certain performance tests devised in America, with special reference to their use in vocational guidance.[3]

The earliest of these reports, which was published in 1921, showed that up to that time all of the investigations, with one exception,[4] had been concerned with vocational selection rather than vocational guidance (that is, with the requirements of particular occupations rather than those of particular individuals), and had been almost entirely confined to the United States and to Germany. It is true that signs of further development were even then apparent, as shown by the foundation in 1920 of the National Institute of Industrial Psychology in this country and by the fact that in other countries, especially those with industrial interests, institutions with somewhat similar functions were in process of formation.[5] These, however, were but initial though necessary steps towards the wide and systematic study of vocational guidance and selection, and little work had been actually initiated outside the countries referred to.

During the last few years there has been a large expansion in the scientific research carried out on these subjects, accompanied by signs of an awakening public appreciation of its future possibilities. In England, in particular, the National Institute of Industrial Psychology made continuous progress towards permanent establishment and was quickly in a position to engage the services of a staff of trained psychologists. At the time,

[1] Muscio, B. (1921) : Vocational Guidance (A review of the Literature.) I.F.R.B. Report No. 12.

[2] Muscio, B. and Farmer, E. (1922) : Three Studies in Vocational Selection. I.F.R.B. Report No. 18.

[3] Gaw, Frances (1925) : Performance Tests of Intelligence. I.F.R.B. Report No. 31.

[4] The investigation referred to is that of the Committee on Classification of Personnel in the U.S Army, which involved vocational guidance within certain defined limits.

[5] An account of the chief of these institutions will be found in 'Vocational Guidance in Foreign Countries' by G. H. Miles (*Jr. Nat. Inst. Ind Psych.* **1** ; 1, 3, 4 ; and **2**, 3) ; and in the 'Report on the Present Position of Vocational Guidance and Selection,' by Miss M. Boole Stott (Women's Employment Publishing Company, Ltd., 54, Russell Square, W.C.1).

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however, it felt itself provisionally precluded from embarking unaided on a long and immediately unremunerative research, and accordingly in 1922 the Board, on the advice of their Committee on Industrial Psychology, resolved to co-operate with the Institute in a joint preliminary investigation, the results of which are embodied in the present report.

As this investigation on vocational guidance was to be the first of its kind carried out in this country, it was decided to confine it to a rather narrow field, and to study a small number of children with thoroughness rather than to attempt a large-scale experiment with the object of obtaining extensive data; in other words, the most complete determination possible of the personal qualities of each individual child and of the inter-relation of these qualities was the object aimed at rather than the estimation of the practical value of the tests as judged by the subsequent history of the subjects tested.

The scheme finally adopted is fully described in the Report. Briefly it consisted of the following three stages:—

I. As a preliminary step, an analysis of the occupations taken up by 1,000 children leaving the schools in a London Borough was made, in order to ascertain which were likely to be the commoner occupations involved.

II. The investigation proper, which was limited to the children educated at three schools in the Borough, consisted of an intensive individual study of all the children due to leave the three selected schools in the course of the next twelve months and of the making of vocational recommendations based on this study.

III. After the lapse of two years as many as possible of the children who had been tested were traced, and the subsequent successes or failures noted and related to the indications of the tests.

The general conclusions are fully set forth on pp. 98–102 of the Report, and need not be repeated here. The Board, however, think it desirable to direct special attention to two points arising out of them.

In the first place, the Board are aware of the opinion sometimes held that the only satisfactory way of determining the fitness of an applicant for any occupation is by an ordinary interview followed by a careful application of the method of actual trial. They desire, therefore, to emphasise the fact that even within the narrow scope of the present investigation one clear indication of the practical value of vocational tests emerges. The supplementary inquiry into the subsequent history of the children tested shows that both stability of employment and contentment with the conditions of work are greater amongst the group in occupations which the tests indicated as suitable than amongst the group in unsuitable occupations.

This, of course, does not imply that the selection of an occupation should depend solely on the application of these tests. Choice is often limited by local circumstances, and in addition there may be other reasons far outweighing any indications derived from vocational tests. Instances, however, must often arise where there is no predisposing tendency towards one of several available occupations, and it is here where vocational guidance methods are likely to be of greatest value. A boy or girl found to be naturally unsuitable for one occupation will almost certainly be found suitable for another. Different occupations clearly demand different aptitudes, and vocational guidance as it progresses will merely indicate to the applicant whether one occupation is more suitable in his particular case than another.

Secondly, the Board fully concur with the authors in their view that one of the most important results of the present investigation has been to show the imperative need for further research. They are therefore glad to know that investigation is being continued on a more extensive scale by the National Institute of Industrial Psychology under a grant from the Carnegie United Kingdom Trust.

The special thanks of the Board are due to Professor Cyril Burt for directing the investigation, to the London County Council Education Authority for permission to examine the children, and to the teachers in the schools selected for much indispensable assistance and co-operation.

February, 1926.

INDUSTRIAL FATIGUE RESEARCH BOARD.

REPORT No. 33.

A STUDY IN VOCATIONAL GUIDANCE

BY

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I. Introduction.

The Problem of Vocational Guidance.—It is the purpose of vocational psychology to discover and measure by scientific means those varying qualities of mind that make different individuals suited to different occupations. Vocational psychology has thus two branches, and pursues a double aim; it seeks to choose the best man for any given occupation, and the best occupation for any given man. The first is the task of what is termed vocational selection; the second, of what has come to be known as vocational guidance. On the problems and methods of vocational selection—the selection, that is, of the most suitable employees for a particular kind of work—numerous investigations have lately been made, and with much success. But hardly a single systematic research has hitherto been undertaken, either in this country or elsewhere, upon the feasibility and the value of psychological methods in guiding young people in their choice of employment. Yet, in the interests both of the workers and of the community at large, this is plainly by far the more important problem of the two.

Accordingly, as has been stated in the preface, an experiment on the possibility of vocational guidance for children leaving the elementary school was planned by a small body of investigators under the joint direction of the Industrial Fatigue Research Board and the National Institute of Industrial Psychology. The experiment has been limited to three schools of a London borough. The district was so selected as to provide a wide variety of typical occupations; and the schools were so picked as to be fairly representative of the average type of London child. The following report seeks to give a brief account of the results obtained. The separate sections are for the most part the work of the individual investigators; but the inquiry as a whole was carried out in accordance with a jointly pre-concerted scheme.

Present Procedure.—Until about fifteen years ago the vital problem of choosing a first employment was left almost entirely to the child and his parents. Valuable advice, indeed, was sometimes given informally by the child's own teachers. Many headmasters and headmistresses were in close touch with the more important firms of their districts, and were often approached by employers as well as by parents for suitable recommendations. Further aid, too, was occasionally rendered by a small band of voluntary workers, interested for the most part in the social rather than the industrial or psychological side. But this was all.

Early in 1910, however, in accordance with the powers conferred by the Labour Exchanges Act passed in the previous year, the Board of Trade issued rules providing that special advisory committees for juvenile employment should be established in such areas as seemed expedient; and a few months later the Education (Choice of Employment) Act was placed upon the statute book. This Act, by its brief clauses, afforded local

education authorities the financial powers that were necessary to enable them "to give boys and girls information, advice and assistance with respect to the choice of employment."[1] In London, in accordance with this Act, the primary responsibility for advising children who left the Council's schools remained, until about a year ago, with the school care committees, working under the general organization of the Education Officer's Department of the London County Council. Between them, however, the two new Acts had set up a dual system of control. It was a system fraught with difficulties in actual administration. Eventually, after Lord Chelmsford's report, the London County Council decided not to undertake the administration of the Unemployment Insurance Acts relating to juvenile workers. As an automatic result, the full and undivided responsibility for dealing with children and young persons in all matters relating to employment passed to the Ministry of Labour; and has remained with it since April, 1924.[2]

So early as 1915 a brief informal study of vocational guidance was made by one or two officials of the London County Council[3] along psychological lines. Owing to the war the experiment was discontinued; but it was sufficient to reveal the need for a fuller investigation, and to demonstrate the main lines upon which such an investigation might be carried out. In particular, it was noted that, under the Acts just cited, there was already in existence administrative machinery which might be made the basis for such work. The machinery, which with slight modifications still continues, has been embodied in a revised scheme of after-care and juvenile employment experimentally adopted for the County of London.[4] Every child, as he leaves the

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[1] The provisions of this Act have since been incorporated in the Education Act, 1921 (Section 107).

[2] In Great Britain the earliest occasion on which the State has recognized the need for giving guidance and aid to children as they move from school to industry is to be found in the Education (Scotland) Act, 1908, an Act passed a year before the labour exchanges were set up. The application of psychological methods to the problems of vocational guidance appears first to have been made in America, and dates from about the same period. A brief history of the subject will be found in the Board of Education's *Report on Psychological Tests of Educable Capacity* (1924, price 2*s*. net; see especially pp. 47–53, 98–100, and the short bibliography, p. 197). A fuller *Review of the Literature on Vocational Guidance*, compiled by B. Muscio, has been published by the Industrial Fatigue Research Board (Report No. 12, 1921, price 1*s*. net).

[3] In this earlier inquiry, carried out under Dr. Burt, assistance was generously given by Mr. J. W. Cox, who devoted his time and energy to the analysis of the accessible records and to the preparation of psychological tests.

[4] The details of this scheme, as provisionally approved, are described in the *First Annual Report* of the London Advisory Council for Juvenile Employment (H.M. Stationery Office, 1925; *see* esp. pp. 10–14). The Advisory Council is a body appointed by the Minister of Labour in March, 1924, to deal, as its title implies, with the question of juvenile employment in London, and (among other problems) with the entrance of children into industry.

elementary school, is briefly described upon a school-leaving form. The form is intended to give a confidential record of the child's characteristics—his home circumstances, his standard or class in school, his physical condition as noted by the school medical officer, his conduct and ability as assessed by the teacher, and, finally, the employment he desires. This form is laid before a school conference, at which the child and his parents attend; and a recommendation regarding the work that seems most suitable is then made and entered. In cases for direct action, through the mediation of the Juvenile Advisory Committee and the Employment Exchange, applicants are submitted to employers forthwith for such vacancies as may be available. To test the recommendation and the placing thus attained, the child is kept under supervision for the next three or four years.

It will be seen that the school-leaving form as finally completed is, or should be, the main basis of the advice so given. Everything, therefore, may turn on the accuracy and fullness with which it is compiled. At present the particulars on the schedules are filled in by teachers during the child's last term at school in a way that is bound to be indefinite and vague: 'conduct—good'; 'intelligence—fair.' As a testimonial such statements may be helpful; as a scientific guide they are of little worth. The list of characteristics and aptitudes, therefore, needs to be carefully elaborated and revised; and the chief entries to be defined in accordance with some 'psychographic scheme,' more precise, more strictly comparable, and more accurately standardized. How far, in the present state of knowledge, this is really practicable is one of the first and most important problems to be solved by the vocational psychologist.

Occupations open to Children leaving School.—Before deciding what aptitudes or qualifications are to be looked for, it is necessary, first of all, to know what aptitudes or qualifications may be wanted in the employments that the children are likely to enter Certainly, no scheme of tests can be drawn up, until it has been ascertained what special vocational abilities will chiefly call for measurement. Accordingly, as a preliminary to the main research, one of the investigators undertook a careful statistical analysis of the occupations actually entered by a large group of children who had recently left school in the district where the inquiry was to be carried on. The results of the analysis are set out in Tables I and II.[1]

[1] This preliminary analysis was made by Mrs. L. Ramsey To the staff of the local office for children's care, who so willingly put their records and their experience at our disposal, we are especially grateful. For help in the further classification of the data so obtained, we are also much indebted to numerous workers familiar with the conditions of the district—teachers, employers, labour exchange officials, as well as the young employees themselves. As is noted in the text, the classification is bound to be somewhat arbitrary and approximate In the same area and in the same office, however, an earlier analysis along similar lines was made by Mr. J. W Cox; and it is encouraging to note that in both inquiries the final percentages agree quite closely, and so corroborate each other.

TABLE I.—CHOICE OF EMPLOYMENT (BOYS).

Analysis of First Situations accepted by 1,000 Boys leaving London Elementary Schools.

Nature of Employment.	*Percentage.*
A. PROFESSIONAL OR SEMI-PROFESSIONAL.	
1. *Higher Education* (without immediate employment)—	
Secondary schools	0·1
Technical schools	0·3
Total	0·4
B. CLERICAL OR HIGHLY SKILLED.	
2. *Office work* (with training or continued education)	3·8
3. *Engineering* (with training or continued education)	4·1
4. *Trade or other schools* (without immediate employment)	0·2
Total	8·1
C. SKILLED.	
5. *Engineering* and allied trades—	
General engineering	2·2
Electrical engineering	1·8
Motor works	1·8
Wireless	0·3
Aeroplane making	0·2
6. *Toolmaking* and allied trades—	
Scientific instrument making	2·8
Optical work	2·1
Miscellaneous tool-making	0·5
7. *Carpentry* and allied trades—	
Cabinet making	2·3
Carpentry and woodwork	2·5
8. *Tailoring* and allied trades—	
Tailoring	0·6
Bootmaking	0·9
9. *Miscellaneous*—	
Printing or lithography	1·5
Photography	0·3
Upholstering	1·2
Leather work	1·3
Watch or jewellery making	0·9
Stained glass work	0·4
Various	1·9
Total	25·5
D. SEMI-SKILLED.	
10. *Shop assistants* and allied work—	
Barbers' assistants	0·5
Chemists' assistants	0·5
Post Office stores	0·3
General stores	0·3
Warehouses	0·6
Shop assistants (various)	5·4

TABLE I.—*contd.*

Nature of Employment.	*Percentage.*
D. SEMI-SKILLED—*contd.*	
11. *Metal and woodwork, etc.*—	
Metalwork	3·4
Brass polishing	1·0
French polishing	1·8
Piano making	2·8
Sawmills	0·9
12. *Miscellaneous*—	
Office boys	2·0
Laboratory boys	0·2
Waiters	0·6
Packers	0·5
Semi-skilled manufacturing (pens, pencils, billiard cues, buttons, artificial teeth, etc.)	4·1
Various	5·2
Total	39·1
E. UNSKILLED.	
13. *Delivery of goods*—	
Errand boys	14·1
Van boys	5·2
Paper boys	1·0
Milk delivery	0·6
Railway porters	0·5
Butchers' boys	0·4
Greengrocers' boys	0·2
14. *Messengers*—	
Post Office messengers	1·7
Cable company's messengers	0·6
District messengers	0·4
Page boys	2·5
Lift boys	0·2
House boys	0·5
Various	0·6
15. *Miscellaneous*—	
Factories (other than skilled or semi-skilled)	3·0
Labourers	2·1
Various	2·3
Total	35·9

Note.—The class-headings given in this and the following table (' Skilled,' ' Unskilled,' ' Semi-skilled,' etc.), have been adopted to mark what seem to be the most natural subdivisions. For terms so vague it is difficult to devise exact definitions. Nevertheless, as will appear later on, these subdivisions run roughly parallel to a simple scheme for the classification of intelligence: and the somewhat arbitrary dividing-lines have accordingly been fixed to correspond with that scheme. Throughout this report, therefore, these terms are used as convenient labels for groups of occupations or trade-processes requiring approximately the grades of intelligence specified in Table IV.

TABLE II.—CHOICE OF EMPLOYMENT (GIRLS).

Analysis of First Situations accepted by 1,000 Girls leaving London Elementary Schools.

Nature of Employment.	*Per-centage.*
A. PROFESSIONAL OR SEMI-PROFESSIONAL.	
1. *Higher education* (without immediate employment)—	
Secondary schools	0·2
Technical schools	0·1
Total	0·3
B. CLERICAL OR HIGHLY SKILLED.	
2. *Office work* (with training or continued education)—	
General	3·9
Shorthand and typewriting	0·5
Book-keeping or cashier	0·6
3. *Dressmaking or showroom work* (for high-class firms)	1·3
4. *Trade or other schools* (without immediate employment)—	
Shorthand and typewriting schools	0·5
Dressmaking or domestic economy	0·3
Various	1·1
Total	8·2
C. SKILLED.	
5. *Office work* (routine)	4·1
6. *Dressmaking*, and allied trades—	
Dressmaking	6·5
Tailoring	3·0
Blouse or collar making, etc.	0·9
7. *Millinery* and allied trades	2·7
8. *Embroidery*—	
General embroidery	2·3
Silk bag making	0·2
Bead work	0·2
9. *Printing* and allied trades—	
Printing	1·2
Bookbinding	1·4
Engraving, stencilling, etc.	0·4
10. *Miscellaneous*—	
Upholstery	2·9
Leatherwork	1·3
Artificial flower making	1·2
Optical work	1·0
Jewellery and jewel case making	1·2
Silk lampshade factory	0·8
Fur trade	0·3
Various	1·4
Total	33·0

TABLE II.—*contd.*

Nature of Employment.	*Percentage.*
D. SEMI-SKILLED.	
11. *Shop assistants*—	
Drapers' assistants	3·2
Hairdressers' assistants	0·4
Post Office (counter work)	0·4
Various	3·8
12. *Domestic service* (superior)—	
General	3·1
Nurses	1·6
Waitresses	0·3
13. *Packing* (delicate goods)	3·7
14. *Seamstresses*	4·2
15. *Miscellaneous*—	
Confectionery	1·5
Cigarette making	1·6
Artificial tooth making	2·7
Paper making	2·4
Toy making	1·3
Laundry work	1·1
Various (chiefly semi-skilled manufacturing)	3·6
Total	34·9
E. UNSKILLED	
16. *Delivery of goods*—	
Errand girls	2·8
Various	1·7
17. *Messengers*—	
Post Office messengers	0·6
Lift girls	0·4
18. *Domestic service* (inferior)	4·6
19. *Factory work* (other than skilled or semi-skilled)—	
Packing (coarse goods)	3·8
Bottling, labelling, etc.	3·2
Miscellaneous unskilled factory work	6·5
Total	23·6

Two thousand consecutive cases were taken—a thousand boys and a thousand girls coming from eighteen representative schools in the borough. The period chosen was the period intervening between the close of the war and the commencement of our inquiry, namely 1919 to 1922.[1] The percentages show the

[1] It might be thought that, owing to unsettled conditions following the war, the period picked for our work was unfortunate. It appears, however, that economic disturbances have not gravely affected the problems of juvenile employment. It has been estimated that, of young persons between the ages of 14 and 18, barely 4 per cent. are unemployed, and, of the children aged 15 to 16, over 62 per cent. have worked in one place only since leaving school, and no less than 90 per cent. have worked in not more than two places. Juvenile employment, therefore, has remained much nearer the normal, and far more stable, than might have been assumed. (See *First Annual Report of the London Advisory Council for Juvenile Employment*, 1924–25, p. 17.)

relative proportions entering each of the occupations catalogued in the table. In an initial investigation it is possible to think only of the commoner forms of juvenile employment. The vague sub-headings 'Various' or 'Miscellaneous' are used where not more than one child in a thousand entered each of the trades so grouped.

The range of occupations is enormous. At the one end of the scale, a large number of the lads become errand boys, van-boys, or paper-boys, doing work that could be done (and often actually is done) by youths who are mentally defective. At the other end of the scale, there are children who have been sent by their parents to secondary schools in the hope that they may subsequently pass to a University, and rise to one of the higher professions. On a first casual glance it might seem that the bulk of the occupations actually taken up could be divided into the two familiar groups—skilled and unskilled. This, however, to the psychologist is rather like bisecting the whole population into the clever and the dull, those who have high intelligence and those who have none. Everywhere in mental classification these easy antitheses are to be doubted; and a closer scrutiny of the occupations shows that the popular grouping, as usual, ignores the immense variety of individual differences. We found ourselves compelled to attempt some finer classification; and it seemed necessary, first, to introduce an intermediate category which we have headed semi-skilled, and, secondly, to recognize the existence at least of a higher and a lower degree of skill. In all we have thus a minimum of five main classes, grouped as shown in the table. Even so, it was far from easy to determine under which general class a given situation should be entered. Often it was essential to consider, not merely the actual work on which the child was to be engaged on first taking up his job, but also the amount of experience needed before he could become proficient in it, and the further opportunities thereby opened up for training, instruction, and advancement to a post of responsibility. After all, what primarily concerns the vocational adviser is not so much to discover a task that seems immediately suited to the child's present powers, but rather to direct him to some permanent employment where he may gradually press forward to a position that shall eventually satisfy his fullest needs.

Averaging the final figures for the two sexes, it would appear that rather less than 10 per cent. enter clerical or highly skilled occupations, and that about 30 per cent. are to be found in each of the groups designated skilled, semi-skilled, and unskilled respectively, the numbers (for the boys at any rate) increasing progressively with decrease in the skill required. As we shall see in a moment, and as might naturally be inferred, this high incidence of low-grade occupation diminishes somewhat with advance of years; and at higher ages the relative proportions tend more and more towards a normal or at least a symmetrical

distribution.[1] Yet always, much as it may be regretted, there seems more room at the bottom than at the top. It must be noted, too, that, since the brightest children leave the elementary school for the secondary at the early age of eleven, and since there were no secondary schools included in our inquiry, the figures for professional or semi-professional callings are in our table disproportionately small: for the whole of London they would amount to between 1 and 2 per cent.

The rough percentages shown in the table can, of course, claim no universal validity. They hold good primarily for the area in which our work was done. Even within London itself, such figures vary widely from district to district; and, compared with the rest of the country, London itself is an exceptional case. As a national warehouse and distributing centre, its peculiar position leads inevitably to an unusual predominance of unskilled employment and of casual labour.

When every allowance has been made, there remains a great variety and range of work accessible to the young child; and throughout, the main distinctions seem to rest largely upon degrees of skill—of skill, not in the sense of acquired dexterity or experience, but of aptitude for learning and for profiting by opportunity. In this sense of the word, skill appears almost identical with what the psychologist terms intelligence; and it became clear at the outset of our research that one of the most important problems for the vocational adviser would be to ascertain the degree of intelligence possessed by each individual child. Within the vocational scale as thus arranged, there are several obvious and outstanding points. For boys, by far the commonest form of skilled work was that which may be loosely designated engineering. For girls the commonest form of skilled trade was that which may roughly be described as needlework or dressmaking. For both sexes the various clerical occupations together make up another distinguishable group, fairly well marked if not quite so numerous; while a fourth big class is formed by that group of occupations which involves waiting upon other people—domestic service among the girls, messenger work among the boys, and among either sex (at a somewhat higher level) serving in shops and stores.

This initial analysis, then, indicated very broadly what would be the most important tests to use, and what would be the most essential qualifications of aptitude or temperament to be looked for.

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[1] According to the Census returns for the metropolis, approximately 10,000 boys about the age of 14 become errand boys and van boys; by the age of 20 there are barely 1,000 left in such occupations. Similarly, in spite of the increasing tendency for manufacturing firms to move to the provinces, an enormous number of young girls are always in demand for unskilled work in factories: marriage, in later years, speedily thins their ranks.

Plan of Inquiry.—The main part of the research consisted of an intensive individual study of all the children due to leave the three selected schools in the course of the next twelve months. The number amounted to exactly one hundred—52 boys and 48 girls; and the study covered each of the main fields of information.

(1) One investigator visited the homes and ascertained the social and economic status of the family, the occupations and the wishes of the parents, and any information that might be likely to throw light upon the hereditary tendencies of the children.

(2) All available data about the child's physical condition and history, his school attainments and school progress, were collected from the teachers and from the cards recording the results of past medical inspections. The results of the medical inspection appeared to be sufficiently accurate to dispense with the need for any special physical, anthropometric, or medical examination.

(3) The mental capacities of the children were tested and studied by a group of investigators, each employing tests upon which they had specialized. The purpose of the tests selected was to measure intelligence, educational attainments, certain specific capacities like mechanical ability or imagination, and natural aptitudes for particular trades like dressmaking.

(4) Each child was subjected to a personal interview, during which an endeavour was made to assess other essential characteristics—for example, emotional, moral, and social qualities—which could not be directly measured by the tests.

In order that each child might be studied systematically, a detailed schedule or 'psychographic scheme' was drawn up beforehand by Dr. Burt, indicating the particulars which it was desired to obtain in every instance. The chief headings are summarized below. The various items specified under the several headings have here been omitted. It will be clear from the later sections, dealing with each topic as it arises, what were the particular details sought for in each case. It was hoped that, with the further revisions and modifications shown to be necessary by practical work, such a schedule might eventually suggest the basis for a vocational *dossier* or 'personal file,' and indicate the general outline to be filled in for any particular child who might in the future be examined for vocational guidance.

VOCATIONAL GUIDANCE.

Schedule for Investigating Individual Cases.

I. HOME CONDITIONS.

II. PHYSICAL CONDITION.

III. MENTAL CONDITION.

A. Intellectual Capacity:

1. *General Intelligence:*

(*a*) Verbal tests:

i. Group tests; ii. Individual tests.

(*b*) Non-verbal tests:

i. Group tests; ii. Individual tests.

2. *Specific Capacities:*
   (*a*) Manual dexterity.
   (*b*) Mechanical ability.
   (*c*) Constructive ability.
   (*d*) Imagination.
   (*e*) Capacities needed for particular trades (dress-making, engineering, clerical work, etc.)
3. *Educational Attainments:*
   (*a*) Reading.
   (*b*) Writing
   (*c*) Spelling.
   (*d*) Arithmetic.
   (*e*) Drawing.
4. *Special Interests:*
   General culture, vocational preferences and experience, amusements, hobbies, etc.

B. **Temperament and Character:**

1. *Emotional Qualities:* (cheerfulness, assertiveness, timidity, bad temper, etc.).
2. *Moral Qualities*: (honesty, industry, reliability, etc.).
3. *Social Qualities:* (ability to co-operate with inferiors, equals, superiors, etc.)

IV. VOCATIONAL RECOMMENDATIONS.

At the close of the inquiry all the data obtained for each individual child were collected together, and vocational recommendations were made by the investigators discussing each child singly in the committee. A brief letter was sent to the parents, stating what form of employment seemed best fitted to their child. After an interval of about two years all the homes were re-visited; and a special inquiry was carried out, to discover how many of the children had obtained employment of the type recommended, and to compare their progress with that of the children unable to follow our advice.

In the following pages we shall take up, one by one, the main headings of the foregoing schedule; and shall discuss, in the light of our results, what seem to be the merits and the limitations of each line of approach. We should add that many of our conclusions will be based, not merely on the brief series of school experiments which form the central topic of our report, but also on the special experience gained by several of our number while working in the Vocational Section of the National Institute. For the opportunities there afforded to us we owe a special debt of gratitude.

Throughout the investigation the most cordial co-operation has been received from the parents, from the teachers, from the Care Committee workers, and from the children themselves; to all of them we express our thanks for the help so willingly

given. Without the general experience and the special information put so freely at our disposal alike by teachers and by the officials of the Education Department, our work would have been impossible. We are further particularly indebted to Dr. C. S. Myers, F.R.S., Director of the National Institute of Industrial Psychology, and to Mr. D. R. Wilson, Secretary of the Industrial Fatigue Research Board, for the care with which they have read our report in proof, and for the advice which they have been ready to bestow at every stage of our inquiry.

II.—The Estimation of Intelligence in Vocational Guidance.

By WINIFRED SPIELMAN and CYRIL BURT.

We have seen reason to expect that, in recommending suitable vocations to individual children, one of the first and foremost considerations will be the level of the child's intelligence. Intelligence is now commonly regarded as a general or central factor entering into every mental performance, and determining efficiency in every form of work. From the very nature of intelligence as thus conceived it follows that a quality of such all-pervasive influence must be of supreme importance for industrial success. For its measurement suitable tests are now available; and probably there is no characteristic of the mind that can be so accurately assessed.

Recent surveys carried out in London schools show that the intelligence of individual pupils varies over a range unexpectedly wide. Between the feeble-minded child, who is removed to a school for the mentally defective, and the scholarship child, who wins a free place in a secondary school, there may be a difference equivalent to ten years of mental growth. Take a random sample of a thousand children, all with a chronological age of 10: the brightest will probably be more intelligent than an average child of 15, and the dullest less intelligent than an average child of 5.

To eliminate the effect of different chronological age, intelligence is usually measured in terms of the mental ratio. This is the unit used in our inquiry. A child's mental ratio is found by dividing his mental age, as ascertained from psychological tests, by his chronological age according to the calendar, and expressing the fraction as a percentage. Throughout the years of school life this ratio remains fairly constant. Generally speaking, the mental ratios of boys and girls in the Council's schools lie between 50 and 150 per cent.

The children selected for our main inquiry numbered, as we have seen, no more than one hundred. It was, therefore, only to be expected that, with a group so small, the range of intelligence would be comparatively narrow. Further, long before the age of thirteen, the brightest children from the elementary schools have been transferred to central schools or secondary schools, and the defective have been removed to special schools. Nevertheless, wide differences were found among the children tested: the brightest had a mental ratio of 134 and the dullest one of 62; this means that, at the age of thirteen, their intelligence was scattered over a range of more than nine mental years.[1] Again and again in our inquiry, we noted, what is so commonly found, that the teacher unconsciously minimizes the degree of these differences; nearly always he underrates the cleverness of the clever and the dullness of the dull. Thus, when recommendations

TABLE III.—*Distribution of Intelligence among Children and Adults.*

(1) Level of intelligence (in mental ratio).	(2) Educational category or school.	(3) Number of children (in percentages).	(4) Vocational category.	(5) No. of Male adults (in percentages).
1. Over 150	Scholarships (University honours)	0·2	Highest professional and administrative work.	0·1
2. 130–150	Scholarships (secondary)	2	Lower professional and technical work	3
3. 115–130	Central or higher elementary	10	Clerical and highly skilled work	12
4. 100–115	Ordinary elementary	38	Skilled work Minor commercial positions	26
5. 85–100	Ordinary elementary	38	Semi - skilled work. Poorest commercial positions	33
6. 70–85	Dull and backward classes	10	Unskilled labour and coarse manual work	19
7. 50–70	Special schools for the mentally defective	1·5	Casual labour	7
8. Under 50	Occupation centres for the ineducable	0·2	Institutional cases (imbeciles and idiots)	0·2

[1] Among the cases applying for guidance at the National Institute the mental ratio has varied from 45 to 175.

are based solely on the school-leaving form, the bright child tends to be placed in a situation that is not good enough, and the dull child in a post needing an intelligence that he does not possess.

How, then, can the vocational adviser fit the choice of occupation to the degree of intelligence thus discovered? It is clear that some guiding scheme is wanted to show the distribution of intelligence among the industrial population, and its relation to what is required in different classes of employment.

A classification of London school children according to their mental ratios is given in the first two columns of Table III.[1] The figures in the third column show approximately what percentage of school children falls within each grade of intelligence. These grades of intelligence correspond in turn to the type of education that the child is or should be receiving; and the percentages answer roughly to the numbers in schools of different types.

Among adults the range of intelligence varies quite as widely as among children. This has been demonstrated by recent examinations carried out with group-tests—for example, in this country by the Civil Service tests for ex-Service candidates, and in America by the psychological tests applied during the war to recruits for the Army. When persons so tested are classified according to their several occupations, it is found that there is a broad correspondence between intelligence on the one hand[2] and vocational requirements on the other. The correspondence is shown by the classification of vocations indicated in the fourth column of Table III. In general it will be observed that the amount of intelligence required by a given type of occupation is roughly proportional to the amount of education, training, or experience required to be proficient in it. The calling of a physician requires five or six years training of a university type; the calling of an elementary teacher requires a shorter period at a training college; the work of a shorthand-typist requires only a year or two's training in a business college. A skilled mechanic requires several years' apprenticeship, and may continue learning after he has taken up his job; a chauffeur can learn to drive a car in the course of a few months; repetition-work in a factory may be learned in a week or so, and is picked up once and for all; and, finally, the unskilled work of the navvy or dock-labourer may involve little or no element of learning whatever.

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[1] Based on the figures given in the L.C.C. Report on the *Distribution and Relations of Educational Abilities* (Report No. 1868, P. S. King & Son, 1917), pp. 18 *et seq.*, *cf.* also *Mental and Scholastic Tests*, pp. 147 *et seq.*

[2] It should be noted that, with older persons of more than average intelligence, a mental ratio is not directly calculable, since hardly any test-scales provide or could provide mental ages above 16. In such cases the measurement of intelligence has first to be obtained in terms of some statistical unit, like the standard deviation or percentiles. But for the sake of simplicity, the marking is here expressed throughout in terms of a mental ratio, the calculation being based on the assumption that an individual's mental ratio and percentile position are constant all through life.

The last column of the same table shows the approximate percentage of persons following trades or professions belonging to each group or class.[1] The percentages for the vocations thus classified evince a marked similarity to the percentages of children falling within successive grades of intelligence. It is true that there is a distinct shortage of positions for the more intelligent, and a large excess of positions demanding abilities of a lower grade. Thus, under existing economic conditions, psychological methods can hope only to reduce, never wholly to remove, vocational maladjustments. For the rest, in theory at any rate, to fit a person's occupation to the degree of intelligence which he inherits seems no impracticable ideal; and it becomes the duty of the community, through its schools or other agencies, first to ascertain what is the mental level of each individual child, then to give him the training or instruction most appropriate to that level, and lastly, before it leaves him, to guide him into the career for which his abilities seem to have marked him out.

By the aid of mental tests the vocational psychologist is now able to measure a child's intelligence with fair exactitude; but the measurements themselves can have no value unless the psychologist also has in front of him an inventory showing what particular careers are best suited to each particular grade. A first approximation to such a list, the best we could compile when commencing our investigation, is set out in Table IV. The table enumerates in detail the commoner occupations belonging to each vocational category. The classification is based partly on the employments followed by individuals tested both in England and abroad during the surveys mentioned, partly on the results obtained in testing applicants for vocational guidance at the National Institute, and partly on similar examinations carried out for various minor inquiries. The purport of this classification must not be misunderstood. We do not mean that because a man happens to be a factory worker doing unskilled

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[1] The classification and the percentages have been taken, with some modification and revision, from the table printed by Burt in a paper on "Some Principles of Vocational Guidance" (*Brit. Journ. Psychol.*, 1924, III, p. 349). For the great labour involved in the detailed calculations on which the figures are based, the investigators are especially indebted to Miss V. G. Pelling, formerly statistical assistant at the Institute. Once more the figures finally arrived at are to be taken as nothing more than the roughest approximation. The proportions have been computed primarily from the figures given in the Census returns for London. Unfortunately the divisions and sub-divisions adopted in the Census render it at times extremely difficult to reclassify the numbers on any psychological basis; sometimes a single figure in the Census lists has had to be split among two or more vocational categories; and the only guide has been the opinion of some expert, familiar with the conditions of the trade concerned, who could state what was the usual distribution of employees so described.

Approximate as they are, we feel that these figures are still worth recording. A rough numerical guide is better than no guide at all, or the use of a mere unformulated impression; and the publication of a tentative table may at least stimulate other investigators to a more exact analysis of the particular problems raised.

work his intelligence is of necessity below the average. The psychologist would be the first to deny such an inference: he knows, if only from his tests, how widely varied are the degrees of intelligence to be found within any single occupation, however narrowly defined. Our compilation seeks simply to indicate the

TABLE IV.—*Classification of Vocations according to degree of intelligence required.*

Class I.—Higher professional and administrative work (mental ratio, over 150):

Lawyer, physician, teacher (university and secondary), author, editor, scientist, artist, civil service clerk (Class I), managing director, company secretary, broker, chartered accountant, architect, analytical chemist, professional engineer.

Class II.—Lower professional, technical, and executive work (mental ratio, 130–150):

Teacher (elementary), civil service (second division), accountant, secretary, executive clerk, dentist, veterinary surgeon, reporter, social worker, factory superintendent, surveyor, merchant, auctioneer, buyer, commercial traveller, technical engineer, designer.

Class III.—Clerical and highly skilled work (mental ratio, 115–130):

Shorthand-typist, book-keeper, bank or office clerk, wholesale salesman, musician, specialist teacher (gymnasium, music, domestic science), small merchant, insurance agent, electrician, telegraphist, druggist, hospital nurse, compositor, engraver, lithographer, draughtsman, photographer, tool-maker, pattern maker, moulder machine inspector, showroom assistant, foreman.

Class IV.—Skilled work (mental ratio, 100–115):

Tailor, dressmaker, milliner, upholsterer, engine, tram and bus driver, policeman, telephone operator, printer, mechanic, turner, fitter, miller, finisher, hand-rivetter, cabinet maker, carpenter, plumber, blacksmith, mason, farmer, shop assistant, cashier, hair-dresser, routine typist.

Class V.—Semi-skilled repetition work (mental ratio, 85–100):

Fairly mechanical repetition work requiring low degrees of skill, poorer commercial positions: barber, welder, tin and coppersmith, driller, polisher, miner, furnace man, carter, bricklayer, painter, carpenter, baker, cook, shoemaker, textile worker, laundry worker, packer (delicate goods), postman, coachman, waiter or waitress, page boy, domestic servant (better class).

Class VI.—Unskilled repetition work (mental ratio, 70–85):

Unskilled labour, coarse manual work: automatic machine worker, labourer, loader, navvy, fisherman, farm hand, groom, slater, chimney sweep, packer, labeller, bottler, porter, messenger, deliverer, lift boy and lift girl, domestic servant (poorer class), factory workers generally.

Class VII.—Casual labour (mental ratio, 50–70):

Simplest routine work, and occasional employment on purely mechanical tasks under supervision.

Class VIII.—Institutional (mental ratio, under 50):

Unemployable (imbeciles and idiots).

average ratios obtained, or likely to be obtained, with tests of the type we are using, and so to suggest in the most tentative way the kind of calling we should be inclined to advise for any boy or

girl reaching the specified ratio. That our tests are imperfect we are only too well aware; and we cannot too strongly insist that the fields to which they have been applied, whether by ourselves or by others, are still far too limited for anything but a provisional and hypothetical scheme.

In the first experiments carried out for this research, the intelligence of the children was assessed by means of what are called group tests—tests, that is, to be answered in writing by the children of the top standards sitting together in class. It was found, however, that this method of testing failed to do justice to the duller children, and was apt to underestimate those whose intelligence expressed itself best of all, not through reading or writing, but rather in practical and concrete work. For wider surveys we still believe that group-testing will be necessary, if only to save time and expense: but it is clear that such methods will always have to be supplemented by individual testing, at any rate for the more doubtful cases which the group-tests themselves would probably disclose. In our main inquiry it was eventually decided to limit the tests of intelligence primarily to individual and oral testing carried out by means of the most recent revision of the Binet-Simon Scale, to supplement this scale by a special scale of performance tests, and to experiment with the possibility of a non-language group-test, such as would be less likely to penalize the poor readers and slow writers.

III. Intelligence Tests.

By Winifred Spielman and Frances Gaw.

(*a*) ORAL TESTS: (THE BINET-SIMON SCALE).

Tests Employed.

The Binet-Simon Tests are probably the most efficient tests of general intelligence devised up to the present. A modification of the Stanford Revision was used in this investigation.[1]

The children were tested individually. A few minutes conversation was held with each to put him at his ease; and then the tests were given. The time required was 30–45 minutes for every child. In the case of a few children, the range of whose answers necessitated a great number of questions, half the tests were

[1] This revision is fully described in *The Measurement of Intelligence* by Terman (published by G. Harrap & Co.). A roneoed version of the Stanford scale revised for English children was already in existence, prepared by Dr. Burt, with the permission of Professor Terman, and the aid of many teachers in the Council's schools; this, with some further modifications, was the version employed.

postponed till the next day in order to prevent fatigue. As a general rule children from the lower standards of the school were taken before children from the higher standards to avoid the risk of one child coaching another.

Results Obtained.

As has been stated above, the mental level of a child is most conveniently expressed in terms of the Mental Ratio—that is the ratio $\dfrac{\text{Mental Age}}{\text{Chronological Age}} \times 100.$[1] The results in Table V and VI are summarized in these terms.

TABLE V.—*Mental Ratios with Binet Tests.*

—	Highest Mental Ratio	Upper Quartile.	Median.	Lower Quartile.	Lowest Mental Ratio.
Whole Group (100 cases)	134	105	95	86	62
Girls (48 cases) ..	134	109	95·5	84·5	63
Boys (52 cases) ..	122	103	95·5	86	62

TABLE VI.—*Averages and Variability with Binet Tests.*

—	Mean Mental Ratio	Standard Deviation.	Coefficient of Variability.
Girls (48 cases) ..	96·8	15·6	16·2
Boys (52 cases) ..	93·9	12·8	13·7

The head teachers and class teachers in consultation kindly gave a ranking of the children's intelligence with which to compare the ranking according to the test results. The correlation (*r*) between these two rankings is as follows :—

Girls' School A	.. ..	·93
,, ,, B	.. ..	·53
Boys' ,, A	.. ..	·86
,, ,, B	.. ..	·83
,, ,, C	.. ..	·57
Average Correlation	.. ..	·74

[1] For example a child with a chronological age of 13 years and a mental age of 14 years (computed from the tests) would have a mental ratio of $\frac{14}{13} \times 100 = 108$. If he had a mental age of 12 years his mental ratio would be $\frac{12}{13} \times 100 = 92$. An average child has a mental ratio of 100.

Not much attention need be paid to the fact that the average mental ratio is below the American standard of 100. Some drop is generally found when the Stanford Revision is used with older English children. But the figures confirm the need for a slight readjustment in the age-assignments for the individual tests, before the scale can be considered valid for London children. It will be remembered, too, that many of the brightest children had left the schools selected with scholarships to secondary schools, while but very few had been removed to a special school for mental defectives.

The distribution shows that the girls surpass the boys at the upper end of the scale, though they are about equal to them at the lower end. The mean mental ratio of the girls is therefore higher than that of the boys, and they show a greater standard deviation and coefficient of variability. However, the number of cases (100) is too small to make any generalisations on the question of sex difference. The results of the tests combined for the two sexes exhibit an approximately normal distribution, which, so far as it goes, is indicative of the fact that they were suitable for the children to whom they were given. The median mental ratio for both sexes is the same, viz., 95·5.

The high correlation between the tests and the teachers' rankings shows that this series of tests serves as a fairly satisfactory measure of intelligence. It is possible, however, that both the tests and the teachers' ranking tend to over-stress linguistic ability. At school, intelligence is of necessity evaluated largely by linguistic expression, and many of the tests require verbal facility. A good vocabulary scores at every age from 10 years upwards. Ability to remember numbers is perhaps given more credit than is its due. But, on the whole, the tests form a very serviceable scale by which to measure General Intelligence, especially when vocational guidance is being given for occupations in which a linguistic type of ability is desirable.

Vocational Significance of the Results.

Once the child's intelligence has been assessed, an important step has been taken towards selecting his vocation. The range of choice now open to him becomes confined within narrower limits. The principle is obvious. The degree of intelligence which the child displays debars him from success in such higher occupations as would call for more intelligence than he possesses, and at the same time makes it foolish for him to seek lower occupations such as could be carried out by others less intelligent than he. Take, for example, such work as shorthand and type-writing. In an investigation on clerical occupations carried out by the Institute, it was found that, in a certain office, the few typists having a mental ratio below 105 failed to give satisfaction even in the mechanical work of copying: clerks with a mental ratio

above this level, but below 120, could do routine typing satisfactorily, but failed in the speed and accuracy of their shorthand and in spelling and display: clerks with a mental ratio between 120 and 135 made excellent shorthand-typists, and the brighter could compose business letters upon their own initiative: clerks with a mental ratio above 135 were generally promoted almost at once to more responsible work, or else became discontented if kept on simple routine work. So for other occupations: for nearly every one there is a lower limit of intelligence below which the employee is likely to fail, and an upper limit beyond which he is likely to find his mental powers only partly absorbed by the duties of his post and consequently running to waste. It will be remembered that deductions based on the mental ratios are theoretically independent of age; hence the limits for children aiming at these particular kinds of work could be at once laid down.

This, then, was one of the main principles adopted in making vocational recommendations. In selecting a suitable career for any individual child whose intelligence is known, a tabulated list of occupations such as that given above[1] is of the greatest value. It will be noted, however, that about the middle ranges of intelligence there is a large percentage of individuals of about the same mental level, and a large number of specific trades and occupations open to them. Here, therefore, it becomes of supreme importance not to rest content with an assessment of intelligence alone, but to make a further study of the child's particular aptitudes and of his special qualities of temperament and character.

(b) NON-VERBAL TESTS.

The Need for Non-Verbal Tests in Vocational Guidance.

There are a number of reasons why non-linguistic or non-verbal tests—tests, that is, which are independent of the use of words or of language—should be employed in vocational guidance. Many occupations depend primarily on specific non-linguistic abilities or on general intelligence expressed in non-linguistic terms. In the preliminary analysis[2] of two thousand children's work-records, it was found that a large number of boys took up skilled or semi-skilled work in manual trades like engineering, while the largest percentage of the girls found employment in some form of dressmaking, in clerical work, or in packing. In all these types of work a certain degree of general intelligence is essential; but, with the exception of clerical work, the tasks are non-linguistic; the worker must handle concrete objects in an efficient way, and his work requires him to deal hardly at all with verbal forms of expression.

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[1] See above, Table IV, page 16.

[2] See above, Tables I and II, pages 4–7.

Again, as we have just noted, the ordinary tests of intelligence, such as the Binet-Simon scale, may give an undue advantage to a child with facility in verbal expression. Further, many tests not only depend on the understanding and use of language, but also presuppose a scholastic attitude, and appeal almost exclusively to bookish interests. While facility in the use of language may usually be taken as evidence of intelligence, the absence of such facility cannot be taken as evidence of lack of intelligence. The child with a marked bent toward non-linguistic activities seldom does himself justice in verbal tests; and for him a series of performance tests may prove much fairer, and may give him a more natural medium of expression. Even for the child with facility in the use of language, it is desirable to use both linguistic and non-linguistic measurements, in order to form as complete an estimate as possible of his interests and methods of expressing himself. Non-verbal tests are almost a necessity in certain exceptional cases, *e.g.*, for children who are deaf, who have speech defects, or who are unfamiliar with English. With another group of children, those termed 'verbalists,' this need is no less real, although less apparent. 'Verbalists' are persons who possess a capacity to use language much above their capacity in other ways. For the detection of this special ability non-linguistic tests must be added, as in verbal measurements alone backward subjects of this class may be graded too high. Hence with all children, normal as well as abnormal, it is advisable to use both linguistic and non-linguistic tests of intelligence.

In this research two modes of measuring intelligence in non-linguistic terms were used—performance tests, and a non-language group test.

(i) Performance Tests.

Among non-linguistic tests of intelligence, performance tests are of primary importance. They are essentially individual tests. Thus they afford the examiner a better opportunity to study his subjects' mental processes than does any non-linguistic group test. A performance test may be defined as a short mental problem which may be presented, and must be solved, in non-linguistic terms. It involves, almost of necessity, a manual response from the subject. It is not, however, a test of specific manual or mechanical ability, but rather measures general intelligence without the direct use of words. In many cases performance tests are counterparts of linguistic tests; and the manual movements necessary in solving the former may be taken as analogous to the writing frequently required in the latter. Movements like writing are only a means to an end; and it would be as erroneous to consider performance tests as measurements primarily of manual dexterity, as to consider linguistic tests requiring writing as measurements primarily of the speed and accuracy of writing.

A scale of fourteen performance tests was used in this study. The scale was one arranged by the psychologists at the Psychopathic Hospital in Boston, Mass., U.S.A. and includes part of Pintner and Paterson's scale of performance tests,[1] part of the U.S. Army scale,[2] and Porteus's Maze Test.[3] The following were the particular tests employed[4]:

(1) Healy Picture Completion, Test I.

(2) Healy Picture Completion Test II.
These two tests consist of pictures in which holes have been cut, removing objects essential to the various actions going on. These holes are to be filled by the child with appropriate insets.

(3) Manikin Test and

(4) Profile Test.
These two tests consist of pieces of wood which, when correctly put together, represent a man, and a man's face in profile, respectively.

(5) Cube Construction Test.
This test involves the fitting together of a number of small cubes with sides coloured red and white, so as to form larger cubes with sides coloured in a definite way.

(6) Dearborn Formboard.
This test is a formboard containing six different types of figures, or insets, of simple geometrical shape, which must be rearranged and fitted into the board in certain combinations.

(7) Porteus Maze Test.
This test consists of seven printed mazes, graded progressively in difficulty, through which the child must find the way.

(8) Cube Imitation Test.
In this test, four 1-inch cubes, placed in front of the child, are tapped by the examiner in several different orders, which the child must reproduce.

(9) Goddard Adaptation Board.
This is an oblong wooden board, containing four circular holes, one of which is very slightly larger in diameter than the other three. The board is turned in several different positions, after each one of which the child must indicate the largest hole.

(10) Substitution Test.
In this test numbers must be inserted as quickly as possible for different types of geometrical figures according to a definite key.

(11) Triangle Test.

(12) Diagonal Test.

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[1] Pintner, R. and Paterson, D. *A Scale of Performance Tests*, D. Appleton & Co., 1921.

[2] Yoakum, C. S. and Yerkes, R. M. *Army Mental Tests*, Henry Holt and Co., 1920.

[3] Porteus, S. D., *Porteus Tests—the Vineland Revision*, Publications of the Training School of Vineland, N.J., No. 16, September, 1919.

[4] For a detailed description of these tests, with results obtained from their application to London children, see Gaw, F., *Performance Tests of Intelligence*, Industrial Fatigue Research Board, Report No. 31, 1925.

(13) Healy Puzzle "A."

These three tests are small formboards, with recesses which must be filled as quickly as possible with insets of various geometrical shapes.

(14) Goddard Formboard.

This is a large formboard, the recesses in which must be filled as quickly as possible with appropriate blocks representing simple geometrical figures.

Since performance tests are only short problems—seldom containing more than five or six steps—it is necessary to use a number of such tests, and to combine the results in order to get a mental age or mental ratio. The method for doing this in the present research has been to translate the scores in each test by means of a table of norms into an equivalent mental age, the median of these mental ages being taken as the child's final score. A mental ratio has then been computed for each child by dividing his mental age (as thus obtained with the performance tests) by his chronological age. These ratios will be referred to hereafter in this paper as performance ratios.

(ii) Non-Language Group Test.

The second non-linguistic method of measuring intelligence used in this study was Pintner's non-language group test of intelligence.[1] This consists of a printed booklet containing six component tests. It is similar to most other group scales that involve the use of pictures and form relationships; and requires the subject neither to say nor to write words, but simply to underline or mark the diagrams. As regards the method by which the problem is presented, this test is almost unique even among group tests, in that the examiner must use no words in explaining the test, and must convey his meaning through gestures, and by various simple diagrams and drawings. Pintner's scale is exceptional in yet another respect; it is suitable for subjects of adult level of intelligence as well as for children, while most group tests which do not involve linguistic responses are suitable only for younger children. For each part of this test there is a time limit; and the scoring is in terms of the number of errors made.

Results.

The results obtained with the non-linguistic tests of intelligence are of two kinds: (1) quantitative results, expressed numerically and concerned with the extent to which various abilities are measured; (2) qualitative results, concerned with the nature of the processes elicited, and with characteristics that cannot be expressed in statistical terms.

The first series of results—those called quantitative—are summarised in Tables VII to IX. Table VII shows the scores made in each of the performance tests by this group of elementary

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[1] Pintner, R. 'A Non-Language Group Intelligence Test.' *J. of App. Psych.*, III. (1919), pp. 199–214.

TABLE VII. *Marks obtained in Performance Tests of Intelligence.*[1]

	Cube Imitation.	Adaptation Board.	Goddard Formboard.[2]	Manikin.	Profile.[2]	Healy A.[2]	Picture Completion I.	Picture Completion II.	Triangle.[2]	Diagonal.[2]	Porteus.	Substitution.[2]	Cube Construction.	Dearborn Formboard.	Average Performance Ratio.
Norm. (Median score expected for children aged 13·0–14·0)	7·5	5	12·5	5	150	36·5	511·5	60	38	31·5	13·5	98	16·5	18·2	Boys 100 Girls 100 Avg. 100
Actual Score (Elementary School Children). (Median score actually obtained by children aged 13·0–14·0)	8	5	13	5	76·5	49	578	62·2	40	35	12·5	99	12	15	Boys 96 Girls 93 Avg. 94
Percentage of Subjects—															
Above norm ..	57	70[3]	57	87[3]	79·5	42	69	54	45	48	12·5	46·5	20	14·5	
Below norm ..	43	30	43	13	20·5	58	31	46	55	52	87·5	53·5	80	85·5	

[1]All the children (52 boys and 48 girls) were given all the tests except the Dearborn Formboard, which was given to 31 boys and to 24 girls only.

[2]In these tests the smallest number indicates the best score and the highest degree of success.

[3]The median score actually obtained by the elementary school children was the maximum score in these two tests. The percentages here given (70 and 87 respectively) refer to the children who passed with the maximum score.

school children; the scores are compared with the norms—that is, the scores to be expected from a group of children aged 13·5, according to the standardization of the tests. As the norms for performance tests were worked out for boys and girls together, the results are not separated for the sexes in this table. The third and fourth lines in the table show the percentages rising above and falling below the norm for each test. In these tests the norm is the median score found for a large number of American children, on whom the tests were standardized. By the definition of a median, the scores of 50 per cent. of these subjects must have been above the norm, while the other 50 per cent. must have been below the norm. Hence, if the tests were perfectly standardized for the present group of children, half of them would have scores above the norm, and half below. From Table VII it will be seen that the scores are approximately distributed in this way for seven tests, *viz.*, Cube Imitation, Goddard Formboard, Healy A., Picture Completion Test II, Triangle, Diagonal, and Substitution. In these tests, then, the children made records as good as could be expected for their age.

The distribution above and below the norm is not equal, nor nearly equal, in the other seven tests. In three of these (the Cube Construction, the Dearborn Formboard and the Porteus Test), too few children make scores above the norm. This may be because these three are too difficult for children of this age, or because the present group of children are backward for their age.

There is some reason to believe that both causes are operative. The norms for Cube Construction and Dearborn Formboard, which were adapted from the U.S Army performance scale, appear too difficult. In the Porteus Maze test, however, this group of children probably shows its sub-normality, since other researches with London School children[1] have shown the standardization of these tests to be satisfactory. In the three remaining performance tests (the Adaptation Board, Manikin and Profile), the percentages above the median are so large as to suggest that these tests are too easy for normal children of 13·5. This is not surprising in the first two. In the Manikin and Adaptation Board the maximum score is considered compatible with any mental age at or above 8, so that these two tests do not discriminate between normal children of 13·5. The Manikin and Adaptation Board are, therefore, included in this series of performance tests mainly as a check on the other tests; applied to most of the children they have but a negative significance; they can be taken into account only when the child falls short of the maximum score. It is difficult to explain, however, why English children should be twice as quick as American in the Profile Test. There seems to be no common toy or scholastic exercise specially familiar to English children which would account for their marked superiority.

[1] *See* Burt, C. *Mental and Scholastic Tests*, L.C.C. Reports, 1921, p. 243.

TABLE VIII.—*Sex Differences in Performance Tests.*

	Cube Imitation.	Adaptation Board.	Goddard Formboard.	Manikin.	Profile.	Healy A.	Picture Completion I.	Picture Completion II.	Triangle.	Diagonal.[3]	Porteus.	Substitution.	Cube Construction.	Dearborn Formboard.
Sex differences in mean scores[1]	+ ·68	+ ·05	+ ·94	+ ·07	+14·58	+4·50	−52·35	−1·23	+31·62	+21·22	− ·18	+ ·51	− ·65	+ ·81
Probable error of differences	± ·20	± ·08	± ·26	± ·06	±12·04	±6·52	±12·58	±2·27	± 5·17	± 8·08	± ·19	± ·35	± ·62	± ·64
Standard deviations—														
Boys	1·63	·59	1·74	·52	81·08	48·68	97·72	14·37	25·03	50·10	1·30	2·73	4·27	3·91
Girls	1·44	·59	2·07	·39	96·32	48·03	89·08	18·84	47·48	67·80	1·55	2·57	4·92	3·11
Sex difference as multiple of standard deviation of the boys	+ ·41	+ ·08	+ ·54	+ ·13	+ ·18	+ ·09	− ·53	− ·08	+ 1·26	+ ·42	− ·14	+ ·18	− ·15	+ ·21
Percentage[2] of boys reaching or exceeding the median of the girls' scores.	50·0	67·3	61·5	84·6	75·0	59·6	80·7	55·8	86·5	75·0	67·3	50·0	50·0	61·3

[1] The boys' mean scores were subtracted from the girls for this line ; a plus sign cannot always be taken as indicating superiority, however, since in certain tests (*see* Table VII.) the smallest number is the best score.

[2] Percentage obtaining a better score, whether greater or less numerically.

Table VIII shows the differences between boys and girls in the performance tests. Although the mental ratio obtained by combining these tests is almost the same for both boys and girls, in the separate tests the sex-differences are sometimes well-marked; in the Profile, Picture Completion I, Triangle and Diagonal tests, the boys excel; in Cube Imitation (if the difference between the means is to be trusted), the girls excel. It may be that the differences between boys and girls in performance tests simply illustrate some of the sex-differences found by Burt, Terman, and others—that while girls excel in tests of a linguistic type, boys excel in tests of a perceptual or motor type;[1] and that girls usually excel in memory and imitation, while boys usually excel in reasoning.[2]

In the non-language group test the average score for the thirty boys tested was 107. In Pintner's results scores of 80 to 100 are given as being 'very good." No age standardization for this test was available.

TABLE IX.—*Correlations of Performance Tests and Non-Language Group Test with other Estimates of Intelligence.*

	Estimates of Intelligence.			
	Mental Ratios (Performance Tests.)	Mental Ratios (Binet Tests.)	Non-Language Group Tests.[3]	Teachers' Estimates.
Correlation with Mental Ratios (Performance Tests)—				
Boys	—	$\cdot41 \pm \cdot07$	$\cdot51 \pm \cdot09$	$\cdot35 \pm \cdot08$
Girls	—	$\cdot48 \pm \cdot07$	—	$\cdot42 \pm \cdot08$
Correlation with Mental Ratios (Binet Tests)—				
Boys	$\cdot41 \pm \cdot07$	—	$\cdot40 \pm \cdot10$	$\cdot71 \pm \cdot04$
Girls .. .	$\cdot48 \pm \cdot07$	–	—	$\cdot73 \pm \cdot05$

[3] This test was given to 30 boys only.

Table IX shows the correlations of the performance and non-language group tests with other estimates of intelligence. The performance tests, the Binet tests, the group tests of intelligence and the teachers' estimates are all correlated appreciably one with another. That the Binet tests correlate more highly

[1] Burt, C. *Mental and Scholastic Tests.* L.C.C., 1921, p. 196.

[2] Terman, L. M. *The Measurement of Intelligence.* J. Harrap & Co., 1919, p. 71.
Burt, C. 'Development of Reasoning in School Children.' *J. Exp. Ped.* V (1919), p. 122; and 'Mental Differences between the Sexes,' *Ibid.* I (1912) pp. 362 *et seq.*

with the teachers' estimates than do the performance tests, and less highly with the non-language group test of intelligence, was to be expected. Both the Binet tests and the teachers' estimates seem to have a marked linguistic bias; while the non-language group test has no such bias, and further resembles the performance tests in that form relationships enter into several parts. It is noteworthy that the correlations between performance and Binet tests, and between performance tests and teachers' estimates, are very nearly the same for boys and girls.

For performance tests, even more than for linguistic tests, to find adequate criteria of intelligence is most difficult. To evaluate non-linguistic tests by comparing them with linguistic tests alone would be futile. It is true that a high correlation between a non-linguistic test and the Binet scale, for example, could be taken as a practically certain indication that the former test measures general intelligence; but the reverse, that a non-linguistic test and the Binet scale do not correlate highly, cannot be taken as evidence that the former does not measure intelligence. The two types of test—linguistic and non-linguistic—seem to measure different aspects of intelligence; and some might be disposed to argue that there is as yet no conclusive evidence that linguistic processes correlate more highly with an individual's inborn general ability than non-linguistic. The other obvious criterion—the judgment of the teacher—also has its drawbacks; it not only depends on the personal impressions of a fallible individual, but is very apt to be biassed by a knowledge of the child's school attainments, which themselves in turn depend upon linguistic rather than non-linguistic abilities.

Further results from performance tests—results which might be called qualitative—will be found in another section under temperamental considerations.

Taken in conjunction with the Binet tests, the non-verbal tests of intelligence were found to be valuable in giving a more comprehensive estimate of intelligence than either type of test—verbal or non-verbal—alone would have given. More particularly, they proved to be of special service in picking out boys and girls with a general non-verbal bent. Many children, whose records in linguistic tests were poor, did much better in the performance tests, and showed latent capacities which, but for these tests, might have been overlooked.

IV. Scholastic Tests.

By May Smith.

The scholastic tests employed were those standardized by Dr. Cyril Burt[1]; and consisted of reading, spelling, arithmetic, writing and drawing tests. The standards have been obtained by the examination of about five thousand children; and norms have been established for each age.

As has been noted above, the children tested in the present research were to leave school within a short period of doing the tests; hence they were all either 14 or nearly 14 years of age. Each was, therefore, examined first with tests that were well within his grasp, namely, those for age 12; and success or failure at these determined the succeeding ones, a continuation either to those for ages 13 and 14 or to those for age of 11 and so on.

Handwriting.—The examination was an individual one and not a group test. The children were asked to write as many times as they could, and in their easiest style, the sentence—"Mary had a little lamb." The number of letters achieved in 2 minutes was the measure of success. Speed alone was taken into account in calculating the correlations; quality and legibility (which may be assessed by comparison with standard specimens) were considered only in those cases in which the child was likely to enter an occupation requiring a clear and legible hand.

Calculation.—To test speed and accuracy in arithmetic each child had 4 sheets on which were printed respectively 50 addition, 50 subtraction, 50 multiplication and 50 division sums. Five minutes were allotted to each sheet, and the child was instructed to work as rapidly as possible until told to stop. The number of correct figures done in the prescribed time was taken as the score. In evaluating the final mark, the four rules were taken together. There were a few cases of marked facility in one rule with weakness in another; some children expressed a decided preference for one rule, though this preference did not invariably represent any special facility with it. The children who had considerable difficulty with arithmetic seemed for the most part to be somewhat fatalistic about themselves; and the remark, "I never was any good at this," sometimes coupled with the explanation that some older member of the family was in the same plight, was frequently made. One cannot avoid the feeling that special disability in one school subject and particularly in arithmetic—a disability too often accepted with an unreasoning acquiescence—would re-pay skilled psychological investigation.

Mental Arithmetic.—The test of oral arithmetic (as distinct from the test of speed in arithmetic) was free from a time limit. For the purpose of this test the standard was the actual number

[1] Burt, *Handbook of Tests for Use in Schools*, P. S. King & Son, Ltd., 3*s*. 6*d*.

of correct answers for the particular age. Individual differences in method of working were more marked in this test than in the others Some children fixed their eyes at a point outside themselves, and saw the figures as a clear-cut mental image, in a few cases with such clearness that they felt no disadvantage as against an actual representation on paper. Others vigorously made imaginary figures on the table in front of them and to an observer at a distance would have appeared as if actually working on paper. Some, and these felt distinctly handicapped, complained that they could not keep the figures still, that as soon as they had worked one " in their heads " and fixed it, it wiped itself out, or flickered. Although perhaps not a necessary factor, certainly a good capacity for visualising figures is an advantage in working oral arithmetic. A few children shot out answers almost as soon as the problem had been stated. This rapid answering seems to be typical of a certain kind of temperament, for while it is sometimes allied with extraordinary accuracy, it was found even more frequently among habitual guessers who were more often wrong than right. Such children were, as a rule, but dimly aware of how they worked. Here, as elsewhere, careful notes were kept of each child's method of working, and often proved suggestive when the time came to decide what kind of employment he should be recommended to follow.

Spelling.—Spelling was tested by a graded vocabulary test No time limit is placed upon the child for this test.

Drawing.—As a test of drawing each child was asked to draw " a man," no special type being suggested. Owing to the obvious effeminacy of the " men " drawn by the girls at the first school tested, at the other schools each child was asked to draw both " a man " and " a woman."

The children all seemed interested in the tests and quite willing to do them. Most of them showed no undue fear of authority as represented by the teachers or (for the time being) by the experimenters.

Table X gives the actual average scores for each school, and their probable errors, together with the standard deviation (S.D.) and the co-efficient of variability (C.V.). The co-efficient of variability gives the variability expressed as a percentage of the mean, and enables a comparison to be made regardless of the actual averages, which differ according to the possible number of marks obtainable. Consideration of these co-efficients indicates that in Reading, Dictation, and Writing, the children vary less than they do in either Oral or Speed Arithmetic. The average variation for Reading and Dictation is about 10 (with one exception); for any of the speed Arithmetic tests the variation is about 30 and is highest for Division. It should be observed that School B (*b*), unlike the other schools, contained several children very much below the average in ability, whether that ability was estimated by Scholastic tests or by Intelligence tests.

TABLE X.—*Marks obtained in Scholastic Tests.*

School.	Reading.			Dictation.			Writing.			Oral Arithmetic.		
	Mean.	S.D.	C.V.	Mean	S.D.	C.V.	Mean	S D.	C V.	Mean.	S.D	C.V
A (g) ..	96·2 ±1·20	8·7	9 1 ±0·92	465 ±4·0	29·2	6·3 ±0·61	207·4±4·10	29·7	14·3 ±1·40	77·5 ±2·49	18·1	23·3 ±2·27
A (b) ..	93·6 ±0·88	8·7	9·2 ±0·94	471 ±1·9	19·2	4·1 ±0·41	175·6±2·61	25·7	14·6 ±1·49	94·0 ±0·89	8·8	9·4 ±0·95
B (g) ..	94·0 ±1·34	9·8	10·5 ±1·02	442 ±7·1	51·5	11·7 ±1·14	204·7±4 75	34·7	16·9 ±1·65	83·0 ±1·86	14·3	17·2 ±1·68
B (b) ..	85·0 ±3·5	21·8	25·7 ±3·97	402 ±20·6	126·5	31·4 ±3·6	169·0±4·44	27·2	16·1 ±1·86	81·0 ±3·69	22·6	28·0 ±3·26
C (b) ..	94·0 ±1·1	6·4	6·8 ±0·83	461 ±2·8	16·5	3·6 ±0·44	179·0±4·33	25·5	14·3 ±1·76	83·0 ±2·16	12·7	15·3 ±1·89
Norms : Boys	91·4	12·3	—	467·7	47·2	—	183·7	52·4	—	94·2	11·2	—
Girls	96·8	14·2	—	476·1	29·1	—	185·4	58·2	—	93·5	10·7	—
Average	—	—	12·23	—	—	11·39	—	—	15·23	—	—	21·64

TABLE X.—*Marks obtained in Scholastic Tests*—continued.

School	Speed Arithmetic.											
	Addition.			Subtraction			Multiplication.			Division		
	Mean.	S D.	C.V.	Mean.	S D.	C V.	Mean.	S.D.	C.V.	Mean.	S D.	C.V
A (g) ..	36·6 ±1·60	11·6	31·4 ±3·06	73·75±3·79	27·5	37·16±3·62	69·4 ±3·67	25·9	37·5 ±3·65	45·1 ±2·97	21·5	47·78±4·67
A (b) ..	37·0 ±1·85	12·8	34·7 ±3·5	72·5 ±2·61	18·1	25·0 ±2·55	66·5 ±3·17	22·0	33·2 ±3·37	56·3 ±3·11	21·6	38·6 ±3·92
B (g) ..	39·0 ±2·00	14·6	37·4 ±3·64	70·6 ±3·69	26·8	38·0 ±3·69	77·3 ±3·10	22·6	29·4 ±2·86	47·4 ±2·78	16·7	35·5 ±3·46
B (b) ..	37·4 ±1·94	12·5	38·5 ±4·45	64·0 ±4·25	26·0	41·9 ±4·85	62·5 ±4·53	20·3	32·5 ±3·76	48·5 ±3·94	24·1	49·6 ±5·74
C (b) ..	36·1 ±1·12	6·6	18·2 ±2·42	55·5 ±2·93	17·2	31·0 ±3·70	55·0 ±2·58	15·2	27·6 ±3·40	40·5 ±3·00	17·7	43·6 ±5·4
Norms: Boys	32·5	9·3		61·7	22·6		66·1	21·0		45·4	19·8	
Girls	30·8	13·1		59·9	24·8		65·4	25·2		44·6	21·7	
Average	—	—	32·04	—	—	34·62	—	—	32·25	—	—	43·03

The connexion between ability in one school subject and ability in another is of special interest, and is best illustrated by a table of correlations. For the purpose of correlation the four rules of arithmetic have been taken together and called speed arithmetic, the emphasis here being on speed and mechanical accuracy as contrasted with the test of oral arithmetic where there was no time limit and where most of the sums were of a problem type.

The verdict of the head teacher on the order of the children for intelligence was also obtained, and has been used in Table XI.

TABLE XI.—*Correlations between Scholastic Tests at the different Schools.*

—	School A. Boys.	School A. Girls.	School B. Boys.	School B. Girls.	School C. Boys.
Speed Arithmetic and—					
Oral Arithmetic ..	·50±·11	·83±·04	·51±·12	·70±·07	·71±·08
Writing	·68±·08	·37±·12	·48±·12	·41±·11	·41±·13
Reading	·48±·11	·59±·09	·43±·13	·31±·12	·09±·16
Dictation	·36±·12	·63±·09	·55±·11	·43±·11	−·04±·16
Headteacher's Estimate	·33±·13	·76±·06	·38±·14	·57±·09	·63±·09
Oral Arithmetic and—					
Writing	·51±·11	·43±·12	·17±·16	·48±·10	·18±·16
Reading	·41±·12	·60±·09	·64±·10	·41±·11	−·29±·15
Dictation	·29±·13	·67±·08	·58±·11	·26±·13	−·23±·15
Headteacher's Estimate	·30±·13	·63±·09	·80±·06	·67±·07	·56±·11
Writing and—					
Reading	·48±·11	·59±·09	·32±·15	·09±·14	·26±·15
Dictation	·24±·13	·60±·09	·32±·15	·13±·13	·44±·13
Headteacher's Estimate	·42±·12	·13±·15	·08±·16	·36±·12	·51±·12
Reading and—					
Dictation	·72±·07	·83±·04	·82±·07	·85±·04	·72±·08
Reading and—					
Headteacher's Estimate	·59±·09	·20±·14	·67±·09	·28±·12	·21±·16
Dictation and—					
Headteacher's Estimate	·54±·10	·26±·14	·57±·11	·27±·12	·26±·15

Scrutiny of the above table will show that the coefficients are fairly consistent from one school to another. In each school there is a correlation of more than ·50 between the two types of arithmetic, an interest in and an ability to manipulate figures being probably the common factor; there are however, individual exceptions. Except in one school, the headteacher's estimate of the child's intelligence and the child's ability to do oral arithmetic seem to be closely connected, the correlation being over ·55. In each school dictation and reading correlate highly, the coefficient being over ·70. There is throughout a fairly close connexion between ability to do one school subject and another.

TABLE XII.—*Correlations of Scholastic Tests with one another and with Tests of Intelligence and Mechanical Ability.*

	Arithmetic (Speed)		Arithmetic (Oral).		Writing		Reading.		Dictation.		Drawing		Mental Ratio (Binet Tests).		Mental Ratio (Performance Tests).		Mechanical Ability.	
	Boys.	Girls.	Boys.	Girls.	Boys	Girls.	Boys.	Girls.	Boys	Girls.	Boys.	Girls.	Boys.	Girls.	Boys.	Girls.	Boys.	Girls.
Arithmetic (Speed)	– –	– –	·54 ±·06	·80 ±·03	·43 ±·07	·50 ± 07	·35 ±·08	·56 ±·07	·37 ±·08	·47 ±·07	·12 ±·09	25 ±·10	44 ±·07	·69 ± 05	·21 ±·09	·51 ±·07	—·14 ± 09	·05 ±·10
Arithmetic (Oral)	54 ±·06	80 ±·03	– –	– –	·05 ±·10	·35 ± 08	·33 ±·08	45 ±·03	·34 ±·08	·38 ±·08	— 01 ±·09	·19 ±·10	58 ±·06	·78 ±·04	·32 ±·08	·28 ±·09	— 05 ±·10	·15 ±·10
Writing	·43 ±·07	50 ± 07	·05 ±·10	·35 ±·08	– –	– –	·29 ±·08	·26 ±·09	·32 ± 08	·46 ±·08	·19 ± 09	·19 ±·10	·05 ±·10	·36 ±·09	—·12 ±·09	·29 ±·09	·11 ±·10	·09 ±·10
Reading	·35 ±·08	·56 ±·07	·33 ±·08	·45 ±·08	·29 ±·08	·26 ±·09	– –	– –	·81 ±·03	·84 ±·03	·15 ± 09	·15 ±·10	·34 ± 08	·43 ±·08	—·02 ±·10	·20 ± 09	·06 ±·10	—·13 ± 10
Dictation	·37 ±·08	·47 ±·07	·34 ±·08	·38 ± 08	·32 ±·08	46 ± 08	·81 ± 03	·84 ± 03	– –	– –	·14 ±·09	·00 ±·10	·16 ± 07	·45 ±·08	—·20 ±·09	·25 ±·09	—·21 ±·09	— 02 ±·10
Drawing	·12 ±·09	·25 ±·10	— 01 ±·09	·19 ±·10	·19 ±·09	·19 ±·10	15 ±·09	·15 ± 10	·14 ±·09	·00 ±·10	– –	– –	·16 ±·09	26 ± 09	26 ±·06	36 ±·09	09 ±·10	·06 ±·10
Mental Ratio (Binet Tests) . ..	·44 ±·07	·69 ±·05	·58 ±·06	·78 ±·04	·05 ±·10	·36 ± 09	34 ± 08	·43 ±·08	·46 ±·07	·45 ±·08	·16 ±·09	·26 ±·09	– –	– –	·46 ±·07	·45 ± 08	11 ± 10	—·03 ±·10
Mental Ratio (Performance Tests) ..	·21 ±·09	51 ±·07	·32 ± 03	28 ±·09	—·12 ± 09	·29 ± 09	—·02 ±·10	20 ± 09	—·20 ±·09	·25 ±·09	·26 ±·06	·36 ±·09	·46 ± 07	45 ± 07	– –	– –	—·14 ±·09	10 ±·10
Mechanical Ability	— 14 ±·09	·05 ±·10	—·05 ±·10	·15 ±·10	·11 ±·10	09 ±·10	·06 ±·10	—·13 ±·10	—·21 ± 09	— 02 ±·10	·09 ±·10	·06 ±·10	·11 ±·10	—·03 ± 10	—·14 ± 09	·10 ±·10	– –	– –

In Table XII are given the average correlations of the various school subjects with one another and also with the Intelligence Tests, the Performance Tests and the Tests of Mechanical Ability.[1] Both speed and oral arithmetic correlate highly with the Binet Intelligence Tests; and there is a fairly high correlation between the Binet tests and dictation.

It is interesting to note that the tests of Mechanical Ability show practically no significant correlation with the scholastic tests. Children weak at scholastic work often prove to have considerable ability in other directions, which, unless it is specifically tested, may pass unrealized; yet it may be an ability of considerable importance from the point of view of earning a living, quite apart from the advantage to a child to find that after all there is something at which he can excel.

Too much stress should not be laid upon coefficients obtained from a group so small. Yet, so far as they go, the correlations are entirely consistent with Dr. Burt's suggestion that educational attainments involve, besides general intelligence, at least three or four specific capacities that are relatively independent.[2] The two literary tests—reading and dictation—yield the highest correlations in the table. The two arithmetic tests correlate almost as closely one with another, particularly among the girls. Drawing seems to stand apart from the rest. Further, it is of interest to note that the two speed tests—of speed in arithmetic and of speed in handwriting—show a kind of cross-relationship. It would, however, plainly be precarious to make inferences from success in one subject to success in the others. Hence, it is clear that, in offering evidence on the child's fitness for particular occupations, the teacher or education authority should never be content with assigning a single all-round estimate for educational attainments generally, but should also indicate in what type of work the child does best or worst—whether in literary subjects, in manual subjects, or in calculation, or in tasks requiring speed rather than accuracy and thought.

Individual Studies.

A comparison between different subjects for individual children can be made by assuming that the average mark for each subject is 100, and so reducing each mark originally obtained to a percentage of the mean. This obviates to some extent the

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[1] For a definition of mechanical ability and the manner in which it has been tested, see below, Section V, pp 39–42.

[2] *Distribution and Relations of Educational Abilities* (L.C.C. Report, 1917), pp. 58–9. In that report the unevenness of particular children in particular subjects is dwelt upon mainly from the standpoint of education. But it is evident that such special abilities are of vocational significance too. As will be indicated later, great unevenness is not at all common; but even a slight superiority or a slight inferiority in one particular branch may in the long run make all the difference to success or failure in the career selected.

difference of scale which arises when the children's performances are simply marked in terms of the number of words or sums correctly written or worked.

From the 48 girls and 53 boys available for this investigation a selection was made of those who fell within the first five places in their respective schools for each subject, which yielded 3 girls and 5 boys, i.e., each of these had a position not lower than fifth for each subject. The marks are contained in Table XIII, the figures being given in terms of a mean of 100.

TABLE XIII.—*Marks of Children showing High Ability in Scholastic Tests.*

Children Tested.	Speed Arithmetic.				Oral Arithmetic.	Reading.	Dictation.	Writing.	Average.
	Addition.	Subtraction.	Multiplication.	Division.					
Boy (1) ..	141	131	122	125	98	101	102	102	115·2
,, (2) ..	185	175	137	187	105	97	100	102	136·0*
,, (3) ..	141	114	105	109	85	100	102	120	109·5
,, (4) ..	135	114	83	106	91	99	100	110	104·7
,, (5) ..	138	101	144	136	96	106	104	127	119·0
Girl (1) ..	212	126	122	85	96	103	102	132	122·2
,, (2) ..	128	164	156	138	95	102	103	160	130·7*
,, (3) ..	162	185	149	106	85	105	101	125	127·2
Average ..	155·2	138·7	127·2	124	93·9	101·6	101·7	122·2	

It will be seen that one boy and one girl (each marked with an asterisk) exceed the mean by 30 per cent. The girls' chief success is in speed of writing; they are below the average in oral arithmetic (only a single child being above average in this test), and above the average in reading, dictation and writing. Boy (2) is a very obvious instance where the special ability thus revealed should be taken into account before the child's career is decided. Such a boy would do well as an accountant or ledger-clerk, but only poorly as a clerk whose chief duties were to read documents, write letters, or take down matter from dictation.

Taking the averages for each subject it will be seen that the earliest rule learnt, and hence probably the most stable—namely, addition—is the one in which all excel. Oral arithmetic, on the other hand, has by far the lowest average: of all the scholastic tests it was certainly the least mechanical, and hence allowed for much individual variation; it is also the one most likely to be upset by unusual conditions.

Table XIV gives the marks obtained in each subject by the children with the lowest averages in the scholastic tests.

TABLE XIV.—*Marks of Children showing Low Ability in Scholastic Tests.*

Children Tested.	Speed Arithmetic. Addition.	Speed Arithmetic. Subtraction.	Speed Arithmetic. Multiplication.	Speed Arithmetic. Division.	Oral Arithmetic.	Reading.	Writing.	Dictation.	Average.
Boy (1)	30	9	23	2	34	34	73	19	28·0
„ (2)	44	41	34	15	37	35	62	18	35·7
„ (3)	82	100	69	92	89	96	98	97	90·3
„ (4)	76	91	68	66	77	82	78	101	79·9
„ (5)	85	94	70	83	90	80	78	97	84·6
Girl (1)	47	68	70	42	44	76	78·5	88	64·2
„ (2)	112	92	80	47	61	78	96	89	81·8
„ (3)	41	66	50	34	62	81	94	86	64·2
„ (4)	75	37	70	55	54	79	85	71	65·7
„ (5)	103	94	108	55	67	67	98	61	81·6
Average	69·5	69·2	64·2	49·1	61·5	70·8	84·05	72·7	

The first two boys are obviously very much below the level of the rest of this group, although, in point of fact, the group includes all those who are at the bottom of their respective schools. They are so gravely retarded that it is doubtful how much comprehension they have of any school subject. Their highest scores are in both cases in that subject demanding the least intelligence, *viz.*, speed of writing. Five of the other children (*viz.*, B.1, 2, G.1, 3, 4) are more than 30 per cent. below normal.

In no case out of the 100 tested was there marked superiority in one subject and marked inferiority in another. Those most nearly approaching this are given in Table XV, but it is rather a question of inferiority in some one subject allied with mediocrity in the others.

TABLE XV.—*Cases of Inferiority in One Test associated with Average Ability in the Remainder.*

—	Speed Arithmetic. Addition.	Speed Arithmetic. Subtraction.	Speed Arithmetic. Multiplication.	Speed Arithmetic. Division.	Oral Arithmetic.	Writing.	Reading.	Dictation.	Range of Variation.
Boy (1) ..	144	113	96	140	98	88	82	94	82–144
„ (2) ..	103	35	68	38	69	100	100	102	35–103
Girl (1) ..	69	48	62	17	54	75	103	100	17–103
„ (2) ..	87	68	48	60	93	83	104	97	48–104

The arithmetic of B (1) is on the whole better than his other subjects : B (2) is singularly weak in subtraction and division, the latter being probably dependent upon the former—indeed, with the exception of addition all his arithmetic is poor, though his other subjects are quite up to the average. G (1) similarly is weak in all the tests of arithmetic but obtains average marks for reading and dictation. G (2) can hardly be described as weak except in the multiplication. As a rule, at this age and with this group, if there is a weak subject, that subject is arithmetic in one of its forms.

It is clear that children showing marked inferiority in the scholastic tests as a whole are not likely to do well in occupations demanding for their success speed and accuracy in the use of numbers or words. Their success may probably lie in other directions : but this can only be determined by tests going outside the range of the ordinary school examination.

Vocational Significance of the Tests.

The value of these standardized scholastic tests to the vocational adviser must be obvious without further comment. Backwardness in reading, slowness or inaccuracy in taking in the gist of printed matter, is an evident disqualification for whole groups of commercial occupations. Confusion in mental arithmetic would be fatal to the shop assistant ; slowness and inaccuracy in paper-calculations would be a hopeless handicap to the bank-clerk or accountant. In spite of the advent of the typewriting machine, a quick, neat, and legible hand is still a necessity for many clerks : and proficiency in drawing is equally necessary for a fashion-plate artist or a draughtsman in an engineer's office. Again and again considerations such as these, based on definitely ascertained test-performances, compelled us to dissuade a boy or girl from entering a career to which he had taken a fancy and for which he was nevertheless entirely unfit. It might be thought that the teacher's opinion would forestall the need for specific testing. But too often the teacher's opinion is founded on mere personal impression, roughly formulated in non-numerical terms, and determined by standards that fluctuate enormously from school to school : all this, indeed, the teachers themselves are the first to point out. On the other hand, tests already standardized, like the foregoing, can be applied not only in school, but also in the office or factory : so that what is required in trade or in business can be precisely ascertained beforehand.[1] We may add that the tests here used seemed adequately fitted to our purpose : we should only be inclined to

[1] To give but one striking instance : at a shorthand institution in the neighbourhood we found that approximately a quarter of the girls were being sent for training as shorthand-typists (often with the knowledge and advice of the school-teacher) who nevertheless were unable to spell many of the stock words of a business-letter and further were demonstrably incapable even of learning such spelling by any ordinary methods within the ordinary period of time.

append to our series a test of English composition,[1] at any rate for individuals proposing to take up an occupation in which ability to draft a letter or frame a report is an essential qualification.

V. Special Abilities.

By WINIFRED SPIELMAN and FRANCES GAW.

(*a*) TESTS OF MECHANICAL ABILITY.

(*b*) TESTS OF CONSTRUCTIVE ABILITY.

In many of the occupations entered by young people, particularly in the engineering occupations taken up by older boys, certain capacities which may be broadly designated mechanical ability and constructive ability seem to be essential. Whether such abilities really exist, how they are to be defined, and to what extent they are truly specific—these are questions that can best be discussed after the tests employed have been described.

Three tests of mechanical ability and one of constructive ability were used in this investigation. Of the tests of mechanical ability, two were Stenquist's Assembling Tests, Series I and III,[2] and one was Healy's Puzzle Box.[3] For estimating constructive ability Kelly's test was employed.[4]

The Stenquist Test, Series I, consists of ten common mechanical objects—such as an electric bell, a mouse-trap, a clothes-pin—the parts of which are given to the child, who is to assemble them. Series III is similar to Series I, except that it is much easier: it includes such objects as a bolt and nut, a trunk castor and a plain push-button. In both of these tests the scoring is based upon the number of objects successfully assembled (or partly assembled), and upon the total time taken to complete the series.

The Healy Puzzle Box is a wooden box with a glass top and four holes cut in the side and bottom. It is fastened by a somewhat complicated system of cords and rings slipped over pegs within the box. The problem for the child is to open the box with the aid of a button-hook which he may insert in the holes. The time taken to open the box determines the score.

Kelly's test of constructive ability consists of a number of small wooden blocks and pegs of different sizes and shapes. The child is told to build whatever he pleases, and to do it as well as he can. Hence he must invent his design, as well as carry it out. His score depends on the excellence or merit of the structures that he builds. Of merit there are many different levels, each illustrated by a photograph of an actual specimen. The examiner must assess the merit of the structures built during the test by comparing them with this standardised series of photographs.

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[1] The difficulties of measuring or marking English compositions have been fully discussed by Ballard (*The New Examiner*, pp. 52 *et seq.*); for possible methods, see Burt, *Mental and Scholastic Tests*, pp. 330 *et seq.*

[2] Stenquist, J. L., *Measurements of Mechanical Ability*. Bureau of Reference, Research, and Statistics, Bd. of Education, N.Y.C.

[3] Healy, W. and Fernald, G. M. 'Tests for Practical Mental Classification,' *Psy. Mon.*, Vol. XIII (1910–11), pp. 1–54.

[4] Kelly, T. L. 'A Constructive Ability Test,' *J. of Ed. Psy.*, Vol. VII (1916), pp. 1–16.

No tests designed primarily to measure manual dexterity were introduced into this study. But indirectly this ability was measured in nearly all the tests of mechanical and constructive ability. In these tests the subject must not only understand and recognise mechanical relationships; he must also have the dexterity, the deftness, the control of his hands necessary to put the pieces together.

TABLE XVI. *Marks obtained in Tests of Mechanical and Constructive Ability.*

	Tests of Mechanical Ability.			Test of Constructive Ability.
	Stenquist, Series I.	Stenquist, Series III.	Puzzle-box.	
Norm. (Expected average score for children aged 13·0–14·0)	51·4 per cent. of boys and girls aged 13·5 equal or exceed score of 54·7	No norms published	American boys and girls: 3′ 54″. English boys: 4′ 8″	Boys aged 13·5 60·0
Elementary School Children's Scores. (Average score actually obtained, children aged 13·0–14·0)	50·0 per cent. of boys aged 13·5 equal or exceed a score of 54·7	77·1 (100 boys and girls together)	4′ 06″ (30 boys)	Boys 98·0 Girls 73·1

All the 100 children were given the Stenquist Test, Series III, and the Kelly Test of Constructive Ability. As the Stenquist test, Series III, proved rather difficult for the girls, the two harder mechanical tests, Stenquist Series I, and the Puzzle Box, were not given to them.

Results.

Table XVI shows the scores of these children in the mechanical and constructive ability tests, as compared with the scores made by other children tested elsewhere. It will be seen that in the Puzzle Box the boys do practically as well as other English boys of the same age,[1] and make a record only slightly lower than that reported by Healy for American boys.[2] This may be, however, because Healy's subjects were of a somewhat higher social status than those in this study.

TABLE XVII.—*Sex differences in Tests of Mechanical Ability and Tests of Constructive Ability.*

—	Mechanical Ability. Stenquist, Series III.	Constructive Ability. Kelly Test.
Sex differences in mean scores[1] ..	−12·59	−24·06
Probable error of differences ..	± 2·06	± 4·99
Standard deviations—		
Boys	18·43	43·70
Girls	11·66	29·83
Sex difference as multiple of standard deviation of the boys.	— ·68	— ·55
Percentage of boys in excess of girls' mean score.	78·8	65·5

[1] Boys' mean scores subtracted from girls'.

Table XVII shows the differences between the sexes in the mechanical and constructive ability tests. The numerical results in the Kelly test may perhaps be rather misleading, since the marking appeared to favour boys rather than girls; and the photographs, by comparison with which the marking is to be done, include pictures of few structures built by girls. It was found that more than half the girls tended to build rather homely domestic structures; while the boys often reproduced some object of a

[1] The data for these latter are taken from 'A Study of Practical Ability,' by M. MacFarlane (*Brit. Journ. Psych., Mon. Sup.*, III. viii, Cambridge University Press, 1925).

[2] Healy, W., *The Individual Delinquent*, (Little, Brown & Co., 1915), p. 108.

mechanical type. Owing to the omission of photographs of structures built by girls, the examiner is forced to mark the girls' performances by criteria which are largely subjective; hence his marking is probably much less accurate for girls than for boys.

In tests similar to those here under discussion, three kinds of non-linguistic processes may apparently be involved; those elicited mainly in tests of mechanical ability, those elicited mainly in tests of motor ability (or more specifically manual dexterity), and those elicited mainly in tests of constructive ability. Each of these is important in occupations of different kinds; and it is, therefore, instructive to note the relation between these four types of processes. There seems to be a fundamental difference between mechanical ability and motor ability. The former can be considered as more of an intellectual function than the latter; mechanical ability involves recognizing, understanding and recombining mechanical relationships. It ranges over a great many different levels; and is the same in kind, if not in degree, whether found in a motor mechanic or in a highly trained engineer. Motor ability, as yet almost as much of an unknown quantity as mechanical ability, may be said to include the various forms of conscious muscular control. The form perhaps which has been studied more than any other is manual dexterity. This term may be used to mean control of the hands and fingers, deftness and skill in manual movements, a kind of neat-handedness or neat-fingeredness.[1] Thus defined, manual dexterity is obviously less of an intellectual function than mechanical ability. An example may make this distinction clearer. One article to be assembled in a part of the Stenquist Test is a mousetrap. To put the parts of the trap together successfully involves two things. First, the subject must see where and how the various levers and springs fasten, and secondly he must actually put them into place and fasten them. For the second part of the task he must hold certain parts of the trap without letting them slip while he puts others together. Thus the first part of this test involves mechanical ability, while the second involves a specific form of motor ability, namely, manual dexterity.

Tests of constructive ability almost always involve either mechanical ability or motor ability or both. The chief way in which non-linguistic constructive ability differs from these two seems to be this: it includes, apparently, another element, the factor of creating something new with the material given. Conceivably certain tests of constructive ability and of mechanical ability may hardly differ at all, except that in the former the emphasis is placed upon the originality of the thing constructed, while in the latter it is placed upon the ingenuity and perfection of the actual process of construction. However, a test of constructive ability need not involve mechanical ability of any high degree.

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[1] Whether there is a central or specific capacity entering into all forms of so-called manual dexterity has been doubted by many investigators: see below, page 57.

In addition to the figures shown in the tables, there are other results, involving all four kinds of non-linguistic processes, which cannot be expressed statistically[1]; these results may be called qualitative. Of these, the chief is the appearance of two types of reaction, or methods of expression, a linguistic and a non-linguistic. Several extreme instances of these two types, which appear to exist independently of sex, were observed. For example, one child who did well in the performance tests and tests of mechanical ability, and whose manual dexterity and constructive ability and record in the dressmaking tests were good, nevertheless did poorly in the Binet Test and in tests of scholastic ability. The home report as to her hobbies, leisure pursuits and general interests was quite in accord with her performances in the tests, e g, her chief forms of amusement were of a practical rather than of a verbal type. In certain types of work, e.g., clerical work, a high mental ratio in the Binet tests seems necessary, while the mental ratio in more practical tests is less important. On the other hand, for work such as domestic service and certain factory operations a comparatively good performance in the manual tests seems desirable, even though the mental ratio in the Binet tests is no more than average or even below average. Hence, it appears essential in vocational guidance, not only to test general intelligence in non-linguistic terms, but also to observe the distinction between specific linguistic and non-linguistic capacities.

(c) Tests of Creative Imagination.

Purposive creative imagination is exercised, when, by a conscious effort, ideas, memories or images are dissociated, modified and recombined to form a new and original whole which fulfils the requirements of a given situation. Some measurement of this ability is useful when giving vocational guidance, for it is obviously desirable in certain occupations—such as involve planning, designing or initiative on however small a scale—whilst in others it is far less essential. It is possible that purposive creative imagination is not a single ability, but is highly differentiated according to the type of material with which the imagination is concerned. It can at any rate be divided into two main groups—linguistic and non-linguistic creative imagination, according as the imagination expresses itself primarily through the medium of words or primarily through the medium of concrete objects.

Description of the Tests.

The tests have been devised by the National Institute of Industrial Psychology. They are group tests—that is, they can be applied to a large number of children at the same time; and

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[1] The hypothesis here put forward as to the existence of a non-linguistic factor, might well be tested by the application of the method of partial correlation to experimental data collected on a larger scale

require about one hour and forty minutes. They were given in two spells of fifty minutes each on two successive days. Tests 1, 3, 5 and 6 were given on the first day; tests 2, 4, 7 and 8 on the second.

The first four tests are designed to measure linguistic imagination, and the second four to measure non-linguistic imagination. Each has been completely standardized in wording and timing; and the instructions are carefully phrased so as to be comprehensible even to backward children of thirteen.

The following is a summary of the several tests:—

A.—Linguistic Tests.

1. *Analogy-making.*—Three examples of analogies were shown on the blackboard, for instance: "King is to Queen as Prince is to Princess." The construction was first explained to the children; and the children were then allowed five minutes in which to make up more analogies of their own: the analogies were to be of the same type as those shown, but as different as possible in subject matter both from each other and from the examples. Four marks were the maximum given for each analogy, 2 for correctness of form, and 2 for originality (*i.e.*, for its difference from the previous analogies).

Very few children failed to understand what was required of them. Some, indeed, showed but little imagination, repeating in other words the same type of analogy—for instance, the masculine feminine relationship given as an example: others displayed marked originality, going over a wide field for the subject matter of their sentences.

2. *Stories.*—A coloured picture-postcard—a reproduction of Edward Waller's painting "Sweethearts and Wives"—was given to each child, with instructions to write short notes for a story of which the picture might be an illustration. After four minutes the children were stopped; and were given another four minutes in which to write notes on a second and quite different story that the picture might also illustrate. A third and yet different story about the picture was then asked for with the same time limit. Twelve marks were the maximum given—4 for each story: 2 marks were given for the amount of imagination shown in each story, irrespective of the other two stories, and 2 for its difference from the other two.

Some children could compose only a single short tale; but many of them produced three different plots, each different from the others. The subjects most commonly suggested were:—

(*a*) Soldiers returning from a victory.

(*b*) Soldiers starting out to battle (with various explanations as to why one is injured or afraid).

(*c*) A prisoner being taken to execution.

3. *Predictions.*—The children were asked to predict what might happen if certain things were changed. As an example eight predictions were given by the examiner suggesting what might happen if it became unnecessary for people to eat and drink, such as:

> " All the shops that sold food would have to close."
>
> " No one would be a cook," etc., etc.

The children were then asked to write down as many and as different predictions as possible in answer to the question, " What might happen if everyone could walk and swim at the rate of 100 miles an hour ? " They were given five minutes for the task. Predictions were then required, stating " What might happen if nobody had any teeth ? " and " How might our life be different from what it is now, if reading and writing had never been invented ? " For each set of predictions five minutes were allowed. Four marks were the maximum allotted for each prediction, 2 for its practicability, and 2 for its difference from the suggestions already offered.

Of all the linguistic tests this was perhaps the most satisfactory. The children almost invariably understood what was wanted of them; and the results were extremely varied, and showed the range and fertility of their imagination. Among the numerous replies to the second question ('What would happen if we had no teeth? ')—some quite obvious, others as ingenious as they were far-fetched—the following is quaint enough to be singled out for quotation: 'We should have to use scissors to cut our nails' !

4. *Metaphors.*—An explanation of metaphors was given with examples. The children were then asked to give as many and as different metaphors as they could for:—

(1) An aeroplane flying quickly.

(2) A very fat person.

(3) A prisoner.

Three minutes were allowed for each set of metaphors. Four marks were given for each metaphor, 2 for its relevance and 2 for its difference from the others.

The abilities required to answer this test satisfactorily are perhaps more specialized than those involved in the foregoing; and a certain proportion of the children failed to realize what was required. Nevertheless the results of the test show a high correlation with those of the other tests.

B.—Non-Linguistic Tests.

5. *Maze Planning.*—A diagram of a maze constructed along the lines of squared paper was shown to the children, and its structure was explained. It was then removed; and a piece of

squared paper with sides seven squares long was given to each child. The children were asked to plan a maze on it, and to make the path from the "gate" to the centre path as complicated as possible. Certain conditions were imposed, such as that the walls were to run along the lines of the paper. A time limit of six minutes was allowed. An objective system of marking was devised, with so many marks for the length of the paths, type of paths, number of by-paths. etc.

This test was understood by nearly all the children. Great variation was shown in the complexity of the mazes. Reasoning and intelligence are important factors for the performance of the test; but it also needs visual creative imagination of a distinctly purposive type.

6. *Designing Teapots.*—The children were told to draw, roughly and in outline, eight teapots, all as different in shape from each other as possible. A time limit of five minutes was given. The maximum number of marks was 40 (5 for each teapot). Two marks were given for original sides (*i.e.*, different in shape from sides already given), 1 for an original lid, 1 for an original spout, and 1 for an original handle. The quality of drawing was not considered in the marking.

Some children simply repeated one or two models; others successfully designed eight different styles of teapot.

7. *Pillar-box.*—The children were asked to imagine themselves the Postmaster-General requiring a new pillar-box very different from those in present use, and with many improvements. They were given four minutes to sketch such a pillar-box, with explanatory notes as to the purpose of the alterations. The designs were classified into six groups that were given marks respectively from 5 to 0. The grouping was made according to the originality and practicability of the ideas, but the quality of the drawing itself was ignored.

It was easy to classify the drawings, and typical samples of the groups were soon found; the marking, however, had the disadvantage of being necessarily subjective.

8. *Postman.*—This test was similar in directions and in marking to test 7, except that the design to be made was for a postman's uniform. The time limit was five minutes.

Each day after the four tests were completed, the children were asked to write down which of the tests they liked best, and which they liked least. This afforded some indication whether their taste lay in dealing with linguistic or with non-linguistic material.

At two schools the tests were repeated with the same children after a week's interval to measure the self-consistency or reliability of the tests. The examples to be worked in the second series were made as comparable as possible with those in the first.

Results.

It is difficult to obtain an independent criterion of the children's powers of imagination with which to compare the results of the tests. The various teachers kindly gave judgments of their pupils' imaginations, but expressed a fear that their rankings were not very trustworthy. They had to base their opinion of linguistic imagination chiefly on the originality shown by their pupils in essays, and of non-linguistic imagination on the originality shown in art-work and wood-work.

The correlations between the teachers' rankings and the rankings obtained by the tests are given in Table XVIII.

TABLE XVIII.—*Correlations between Teachers' Rankings and Rankings by Tests of Imagination.*[1]

School.	General Creative Imagination.	Linguistic Imagination	Non-Linguistic Imagination.
A (girls)	·88	·93 [2]	·64 [2]
B (girls)	·23	·22	·24
A (boys)	·58	·71	·34
B (boys)	·63	·73	·53
C (boys)	—·26	—·31	—·43

[1] For the correlations given in this and the three following tables, the probable errors vary as follows:—

School A (girls): from ± ·01 (for a coefficient of ·95) to ± ·08 (for a coefficient of ·64);
School B (girls): from ± ·07 („ „ „ ·74) to ± ·15 („ „ „ ·04);
School A (boys). from ± ·03 („ „ „ ·90) to ± ·16 („ „ „ ·09);
School B (boys): from ± ·03 („ „ „ ·89) to ± ·18 („ „ „ ·05);
School C (boys): from ± ·13 („ „ „ ·46) to ± ·17 („ „ „ ·05);

[2] At this school the teachers gave a general ranking for creative imagination, and did not differentiate it into linguistic and non-linguistic types.

Correlations with other capacities are given in Table XIX.

TABLE XIX.—*Other Correlations for Tests of Imagination.*

School.	Total Creative Imagination Tests and Intelligence (Binet-Simon Tests).	Total Creative Imagination Tests and Intelligence (Performance Tests).	Teachers' Ranking for Imagination and Intelligence (Binet-Simon Tests).	Teachers' Ranking for Imagination and Teachers' Ranking for Intelligence.	Tests of Linguistic Imagination and Non-linguistic Imagination
A (girls)	·87	·64	·85	·71	·73
B (girls)	·71	·68	·19	·74	·51
A (boys)	·61	·35	·70	·88	·64
B (boys)	·61	·40	·78	·89	·65
C (boys)	·46	·35	—·14	·46	·05

No doubt the imagination-tests in their crude results, and probably the teachers in their estimates of their pupils' imagination, are both unintentionally influenced by the factor of intelligence. Accordingly, by applying the device of partial correlation[1] to the inter-correlations of the creative imagination tests, the Binet-Simon tests, and the teachers' ranking for creative imagination, an attempt was made to eliminate this common factor. The partial coefficients (which may be taken as indicating the real influence of imagination as a specific ability) prove to be as follows:

School A (girls)	·54 ± ·10
School B (girls)	·14 ± ·15
School A (boys)	·27 ± ·15
School B (boys)	·31 ± ·15
School C (boys)	·22 ± ·16

The reliability as measured by the correlation between the two applications of the tests to the same children is shown in Table XX.

TABLE XX.—*Reliability coefficients for Tests of Imagination.*

School.	Total Creative Imagination.	Linguistic Imagination.	Non-linguistic Imagination.
A (girls)	·95	·93	·74
A (boys)	·90	·75	·54

In all the linguistic tests the girls are better than the boys, especially in "*analogy-making*" and "*predictions*" where the lower quartile mark of the girls is higher than the median mark of the boys. In the non-linguistic tests the girls are slightly better at designing pillar-boxes and postmen's uniforms, and the boys at designing mazes and teapots.

In three of the departments there is a high correlation between the teachers' rankings and the test results; but those from School B (girls) and School C (boys) give low and negative correlations respectively. The fact that good positive results are sometimes obtained suggests that it may be the teachers' estimates rather than the tests that are at fault, more especially as with the same two

[1] The formula used was

$$r_{12\cdot3} = \frac{r_{12} - r_{13}\, r_{23}}{\sqrt{1 - r^2_{13}}\,\sqrt{1 - r^2_{23}}}$$

where r_{12}, r_{13} and r_{23} represent the total coefficients as originally obtained, and $r_{12\cdot3}$ represents the calculated partial correlation between capacity 1 (imagination as assessed by the tests) and 2 (imagination as assessed by the teacher) after the influence of 3 (intelligence as assessed by the Binet-Simon tests) is eliminated.

schools low correlations were obtained between the teachers' ranking for general intelligence and the well-established Binet-Simon tests.

The tests of creative imagination show a high correlation with general intelligence as measured both by linguistic and non-linguistic scales. The fact that the teachers' ranking for creative imagination does so too (except in the two departments mentioned above) suggests that there is a real relation between intelligence and imagination, apart from the apparent connection so often induced by the testing itself. Nevertheless, there seems also to be a specific factor in imagination which is to some extent independent of intelligence. This view is borne out by the presence of the four positive correlations found remaining between the results of the imagination-tests and the teachers' ranking for imagination after the factor of intelligence is eliminated. It is true that the groups are too small for the correlation to be clearly significant except in School A (girls) and perhaps School B (boys). The problem needs further investigation with larger groups and with more reliable rankings from more numerous judges.

In tests such as these it is impossible to exclude altogether a subjective factor in the marking ; but the high self-consistency of the tests shows that in practice this has been partly overcome. Some objection has been raised to the restricted time limit for a test of imagination. In a few cases this criticism may be valid ; but it must be remembered that the tests have a specifically vocational intent, and in industry purposive creative imagination has frequently to be exerted under temporal restrictions.

Questionnaire on Purposive Creative Imagination.

A questionnaire on purposive creative imagination is useful as a supplement to the tests. It gives some indication of the degree to which the subject has used his imagination in the past, and it partly obviates the difficulty of time restriction that has already been noted. It is also of value in suggesting certain temperamental qualities.

The following questions were read out to the children, giving them time to put a short answer (such as " yes," " 9," " copying ") to each question. Each question was repeated twice. The children were assured that their answers would be shown to no one whom they know :—

(1) *Have you ever written any stories out of school? About how many have you written out of school during the last year?*

(2) *Do you ever paint or draw out of school? Do you like copying pictures or do you prefer making up new ones?*

(3) (*For Boys*) *Do you ever play with Meccano, or with any wooden building game? If so, do you like copying designs or do you like making up your own designs?*

(*For Girls*) *Do you sometimes make up dresses in your head, just for fun? If so, about how many do you make up in the course of a year?*

(4) *If you have to do a certain job, do you like planning it yourself, or do you prefer someone to explain to you all the details of what you have to do?*

(5) *When you go to work, would you like to get the sort of job which, once you have learned it, you know, and need not bother about any more? Or would you like to get the sort of job where you have always to be thinking and planning out new things?*

(6) *What work would you like best to enter? Is there any chance for you to use your imagination in the work you have just chosen? If so, how?*

These questions took about fifteen minutes to answer. The children were then given the following typewritten sheet of questions. The examiner went round the class while they were writing to ensure that the questions were understood.

You will have 30 minutes to answer all the following questions. You need not copy out the questions; just write down the number of the question before you answer it. Your answers will be kept private. Your name will not be given to anyone. You are to answer all *the questions as fully as you can—do not stop to do your best writing.*

(7) *Have you ever written any poetry out of school? Write down the best poem that you have ever written.*

(8) *Have you ever imagined a room just as you would like it best? What was it like?*

(9) *Do you often try to invent things? Describe the most important thing that you have ever invented.*

(10) *If you could be very good at doing something, what would you choose to be good at? Why?*

(11) *In what ways do you enjoy using your imagination?*

(12) *When you read a book do you sometimes imagine that you are the hero or heroine? What kind of hero or heroine do you like being best?*

(13) *Do you sometimes tell a story to yourself at night or when you are alone? Does this vary from night to night, or is it generally the same story continued? What is it generally about?*

Questions 1, 7, 12 and 13 are framed to discover a predominantly linguistic imagination, and questions 2, 3 and 8 to discover a predominantly non-linguistic imagination. Questions 4, 5, 6, 9, 10 and 11 allow of choice in the type of imagination given in the answers; and from these some indication may be obtained as to which is prevalent.

The chief value of the questionnaire lies in the nature of the replies; but certain quantitative information can be gained by marking the answers, giving two marks for each answer in which decided imagination is shown, and one mark for each answer in which slight imagination is shown. Such marking affords a rough numerical measure.

TABLE XXI.—*Correlations of Marks obtained in the Questionnaire with results of Imagination Tests, Intelligence Tests, and Teachers' Rankings.*

School.	Total Creative Imagination Tests.	General Intelligence Tests (Binet-Simon).	Teachers' Ranking for Creative Imagination.
A (girls)	·75	·67	·68
B ,, ...	·21	·04	·46
A (boys)	—·09	·09	—·34
B ,, ...	·11	—·07	·05
C ,, ...			—·16

TABLE XXII.—*Sex-Differences in Questionnaire on Creative Imagination.*

	Linguistic Questions.				Non linguistic Questions			General Questions					AVERAGE.			
	1	7	12	13	2	3	8	4	5	6	9	11	Linguistic	Non-linguistic	General	Total.
Boys' marks expressed as per cent. of girls' marks ..	41	13	106	96	86	104	61	78	86	64	144	95	70	81	89	84

Much information can be gained from the nature of the answers quite apart from their marks, the value of which is necessarily arbitrary. The answers of questions 8, 9, 12 and 13 are particularly suggestive.

Results.

There is a distinct sex difference in the value of the questionnaire. The boys' results show no correlation with creative imagination as measured either by the tests or by the teachers' estimate; but the girls' results give some correlation with both these standards. The girls gain much higher marks than the boys, especially in decidedly linguistic questions such as writing stories and poems or describing a room. The boys, however, easily beat the girls in their account of an invention.

The diagnostic value of the marking of the questionnaire is open to question; but the answers given by the children certainly afford some indication of their temperamental qualities. It must be remembered, however, that great reliance cannot be placed on any form of questionnaire, for some children shrink from giving themselves away, and hence sometimes give colourless or non-committal answers, while others make the most of those qualities that they expect to be desired by the examiner.

(*d*) Tests for Dressmaking.

In the preparatory analysis of the occupations secured by children leaving elementary schools in the district, it was found that 13 per cent. of the girls became dressmakers. This is a larger percentage than that discovered for any other single occupation. In addition about 15 per cent. take up one or other of the allied trades (millinery, embroidery, needlework, etc.). In order to give vocational guidance to girls it is, therefore, important to be able to predict success at these trades. In any one class, where girls have for some time been taught sewing by the same mistress, it may be possible to judge ability directly, by a comparison of their actual work. Even in such a case, however, the comparison is not very just, for girls vary exceedingly in their practice at home: some do a great deal of mending and even making; and others rarely touch a needle. If girls from different schools, or from different classes in the same school, are being compared it is of little use to judge by their needlework itself, for this is so enormously influenced by the teaching. At one trade school two exactly parallel classes had been taught needlework for the same length of time by different mistresses. One class worked more than twice as efficiently as the other even at such a simple stitch as the running stitch, although by the speed tests the two classes were shown to be similarly gifted in capacity for speed. It is, therefore, desirable to use a series of tests to measure the fundamental capacities underlying dressmaking ability, quite independently of skill acquired from instruction or from practice. Tests of this kind have been devised for the National Institute of Industrial Psychology; and were employed in this research.[1]

Description of the Tests.

An analysis of the psychological qualities needed by a dressmaker's apprentice shows that they can be grouped under three main headings :—

A. Those affecting speed of work;
B. Those affecting quality of work;
C. Those affecting ease of learning work.

A. *Factor affecting speed of work.*—The maximum speed is not so important as the usual speed—the proportion of the maximum at which the girl ordinarily works; and this again is affected by the ease with which she is distracted. Distraction plays an important part, since, in most work rooms, discipline is slack, and the girls are free to talk and sing.

B. *Factors affecting quality of work.*—The factors affecting the quality of the work are naturally complex; but they can be divided into two main groups, those which make for lightness of touch and those which make for accuracy of work.

[1] For fuller details *see* W. Spielman, "Vocational Tests for Dressmakers' Apprentices," *Journ. Nat. Inst. Indust. Psychol.*, Vol. I, No. vii.

A light touch means gentle pressure, the avoidance of crushing delicate material. Incidentally, although a light touch is an important asset to a dressmaker, it is of little use to a tailoress, and constitutes one of the chief psychological differences in the requirements of the two trades.

The accuracy of the work depends partly on co-ordination, of hand and eye (when the needle can be seen), and of hand and image (when the needle is brought up from underneath the material). It depends also on a just perception of equal distances, for this capacity enables a girl to fold her work correctly (as in hemming, or making tucks) with greater ease and exactitude, and helps her to make stitches of equal size in handwork and to keep a straight line in machine work.

C. *Factors affecting ease of learning work.*—The factors described above affect the needlework directly; but a girl's success as an apprentice largely depends on the ease with which she learns; and this in turn must depend upon her power of observation and her memory for instructions.

In this occupation, as in every other, temperamental factors must be taken into account. Little advance has as yet been made in measuring them exactly; hence, they are not explicitly included in this analysis; but they were considered when advice was given about entering the trade.

Of the special physiological qualifications, the most important is eyesight; some dressmakers, too, emphasize the importance of having naturally dry hands, but a deficiency in this direction can be partly obviated by the use of French chalk. Many employers choose their girls according to the shape of their fingers. This is a highly arbitrary method of selection: and experts differ as to the desirable characteristics, although all agree as to the value of long flexible fingers. It is quite possible that the shape of a girl's hand may affect her capacity for good work; but it is surely fairer to measure the capacity itself by a psychological test of maximum speed and hand-and-eye co-ordination rather than to attempt indirect deductions from anatomical observation.

Nine tests have been devised to measure the factors given in the above analysis. They can be applied to a group of twenty-four girls in 55 minutes. They are given under standard conditions of order, wording, timing, and apparatus; and the marking for all of them is "fool proof." Their order is so arranged that, as far as possible, the mental tests alternate with the manual, and the harder tests with the easier.

The following were the methods actually employed in the present investigation. All of them aimed at measuring natural aptitude rather than acquired skill due to practice or experience.

Speed tests.—The test for "ordinary" speed and distractibility was the first test given; it was applied before the girls had got into the "set" of expecting speed tests. They were asked to make knots in a piece of wool at their own rate; and were told

"there is no hurry": at the same time the examiner tried the effect of distraction by explaining the purpose of psychological testing.

The tests for maximum speed were (1) threading beads, and (2) pricking with a mounted needle.

Tests for the quality of the work.—The candidate's power of discriminating equal distances was measured by two tests. One consisted of showing her 32 pairs of lines, of which some were exactly parallel, and some nearly parallel but not quite. She had to select those pairs that were exactly parallel. In the other test she was given a limited time in which to judge first the centre of a line, and then the centre of the halves and of the quarters.

The test for co-ordination of hand and eye consisted in aiming with a pencil at certain points on squared paper, and that for co-ordination of hand and image in aiming with a pin from underneath the paper at similar points. In both tests the time was regulated.

Lightness of touch was judged by the degree to which the candidate crumpled new tissue paper in folding it as if for a hem.

Tests for ease of learning work.—Two tests were given to discover the candidate's probable facility in learning her work. Her memory for complicated instructions was measured by reading to her a description of a dress, and finding how much of the description she could remember after a quarter of an hour's interval. Her powers of observation were gauged by showing her two fashion plates for 45 seconds, removing them and seeing how complete a description of them she could give.

Results.

In computing the final ranking for dressmaking ability the results of the various tests were weighted according to their relative importance. The results of the tests for non-linguistic creative imagination were also taken into account when considering the girl's general capacity for this trade. The strength of her imagination does not much affect her success as an apprentice; but it gives some indication of her chances of rising to the higher positions in the trade.

TABLE XXIII.—*Correlations of Ranking by Dressmaking Tests with other Estimates.*[1]

School.	Teachers' Ranking for Dressmaking.	General Intelligence (Binet-Simon Tests).	General Intelligence (Performance Tests).
A (girls)	·71	·70	·75
B ,, ...	·53	·29	·44

[1] For the correlations given in this and the two following tables the probable errors vary as follows:—

School A: from ± ·01 (for a coefficient of ·97) to ± ·14 (for a coefficient of ·26);
School B: from ± ·11 (for a coefficient of ·53) to ± ·15 (for a coefficient of ·05).

Table XXIV.—*Correlations of Teachers' Rankings for Dressmaking with Test-results.*

School.	Speed Tests.	Tests of Equal Distance.	Tests of Co-ordination.	Tests of Ease of Learning	General Intelligence (Binet-Simon Tests).	General Intelligence (Performance Tests).
A (girls) ..	·49	·26	·43	·74	·97	·63
B ,, ..	·44	·31	·38	·35	·05	·35

Table XXV.—*Correlations of Tests for Non-Linguistic Creative Imagination with Estimates of Ability in Dressmaking.*

School.	Dressmaking Tests.	Teachers' Ranking for Dressmaking.
A (girls)	·70	·70
B ,, ...	·32	·35

Table XXVI.—*Relation between Rankings in the Tests and Desire to become Dressmakers.*

Desire to become Dressmakers	Average Rank in a Group of 30.	
	In Dressmaking Tests.	In Teachers' Ranking for Dressmaking.
School A: Girls answering Yes	13	11
,, ,, ,, No	9	10
School B: ,, ,, Yes	10	9
,, ,, ,, No	8	9

The tests yield a fairly high correlation with the teachers' ranking for ability in dressmaking. The reasons why the coefficient is not even greater may be sought partly in the imperfection of the tests, and partly in the fact, already noted, that the teacher's estimate of present ability in needlework depends on the experience as well as on the aptitude of her pupil.

A decided positive correlation is also shown between the teachers' rankings in needlework and the tests for each of the more specific abilities distinguished in the analysis. This relationship, to some extent, bears out the analysis.

The dressmaking tests correlate fairly highly both with tests of general intelligence—particularly of the performance type—and with non-linguistic imagination. The fact that teachers' estimates also give somewhat similar correlations with these two groups of tests, suggests that dressmaking ability is largely dependent both on general intelligence and on non-linguistic creative imagination.

An attempt was made to ascertain whether the girls were selecting their careers according to their abilities. The occupations which they intended entering were tabulated in response to Question 6 of the Creative Imagination Questionnaire. The average rank according to both the Dressmaking Tests and the Teachers' ranking for Dressmaking was found, first for the girls who intended entering the needlework trades, and then for the girls who intended entering other occupations. It was discovered that the average rank in the tests was lower for the girls who intended becoming dressmakers than for those who did not. A possible explanation of this curious reversal is that the cleverer girls, though they may also be good at dressmaking, generally prefer to become clerks; and, as there is a positive correlation between general intelligence and dressmaking, this lowers the average level of the girls who wish to become dressmakers. The difference, however, is small. But, whatever its cause, the reversal—or, at any rate, the absence of agreement—indicates the need for more accurate vocational guidance; and, in many cases, we found it necessary on the basis of the tests to dissuade some of the girls who wished to be dressmakers from entering a trade for which neither the general intelligence nor their special aptitude was adequate.

Other Special Abilities.

We cannot, of course, pretend that the special abilities tested and measured in our brief research cover the whole range of mental differences. In our original scheme, for example, it was planned to introduce tests of engineering abilities for the boys, analogous to the tests of dressmaking for the girls: and to employ, with both sexes, special tests for memory, manual dexterity, and æsthetic appreciation. Time, however, and the resignation of one of our voluntary assistants, forced us to be content with a shorter list of tests. But we believe that our analysis of the aptitudes needed for one sample trade, namely dressmaking, has been sufficient to demonstrate what is needed, and what might be attempted with equal success, in other trades entered by young persons.

At present but little is known as to which abilities are more or less general, and which are more or less specific, and what may be the best modes of testing each one. Vocational advisers are constantly making tacit assumptions on these points. Teachers, for example, often recommend what they call the 'manual type' of child to enter for some form of 'manual' occupation. Such a recommendation implies that there is a

specific capacity, which is common to all forms of skilled manual labour, and to which the term manual ability might fairly be applied. It is quite possible, however, that the various operations, which on a superficial view might all be designated 'manual,' actually depend upon very different aptitudes, and call into play mental and neuro-muscular processes almost wholly uncorrelated one with another. Here, therefore, it is plain that further research is urgently needed. A preliminary series of job-analyses might, indeed, disclose what seem to be similar capacities entering more or less into many different forms of employment; and further experiments with psychological tests might ultimately show whether or not the same capacity was really common to them all.

At present the best opinion inclines to the view that intelligence is the only capacity which has a wide degree of generality, the only capacity, that is, which pervades a large variety of mental processes. Other factors—' group-factors ' as they are sometimes called—appear to be highly specific; and must be consequently as numerous as they are limited. Should this view be confirmed, it would seem at first sight to increase enormously the difficulties of vocational guidance: for time will never permit of every individual child being tested for each distinct ability. The best solution would probably be to divide the examination into two main portions; and to devise first a short series of group-tests for intelligence, for school attainments, and perhaps for what Dr. Burt has called 'key-qualities,' and afterwards to examine the individual children separately with different tests of specific capacity varying according to the particular needs of each.

VI. The Estimation of Character Qualities in Vocational Guidance.

By Winifred Spielman and Cyril Burt.

The Need for Temperamental Assessments.

To succeed in after-life a boy must have character as well as intellect. This, indeed, is a speech-day commonplace. It is not so well recognised that for different callings very different temperaments are needed. The man who becomes restless and discontented behind an office desk may take readily to an active life ' upon the road '; the youth who is too shy to make his way as a salesman or commercial traveller may do well as a mechanic or an engineer where he has to deal not with persons but with machinery. Hence, in advising young people what careers they should choose, it becomes essential, so far as it is practicable, to take into account, not their general intelligence and special aptitudes alone, but also their moral and emotional characteristics.

Tests for such qualities as these, framed upon the same lines as tests for general intelligence, have recently been investigated upon an increasing scale.[1] In the present research, however, it was eventually decided to make no use of these newer devices. Preliminary investigations showed that, in their current forms, tests of temperament are neither exact enough nor self-consistent enough to be of much practical service. Their validity is roughly on a level with that of intelligence tests fifteen or twenty years ago. Their self-consistency and accuracy are sufficiently promising to make further theoretical research upon their nature eminently worth while, but are not sufficiently high to warrant the immediate application of such methods to problems of vocational guidance.

It was, accordingly, agreed to rely upon personal observations rather than upon experimental tests. Unfortunately, but little work has been done hitherto to discover how far such observations agree with one another and with the best ascertainable evidence. It became essential, therefore, as a preliminary, to see whether these simpler methods could claim any higher measure of validity than existing temperamental tests.

Character Estimates by Ordinary Methods.

In the earlier inquiry on vocational guidance in London schools, the following brief investigation was included. Its object was to gauge at the outset how far the customary mode of judging character could be trusted. Two competent observers were selected, both thoroughly experienced in judging the characters of children with a view to recommending suitable employments. They were invited to make records of the character-qualities of a small group of boys and girls about the school-leaving age; and were allowed to choose whatever character-qualities they wished, and to follow whatever method they preferred. Three stipulations alone were made: first, that the character-qualities should have a vocational bearing; secondly, that the same children and the same character-qualities should be judged by both observers; and, thirdly, that the judgments should be put into some numerical form lending itself

[1] Hitherto tests of moral or emotional characteristics have been used chiefly in the study of the neurotic and the delinquent. For a detailed description, and a brief discussion of their value, the reader may be referred to *The Young Delinquent* (University of London Press, 1925, pp. 399–419), where some of the methods here used are set out at greater length. The majority of such tests have been devised and employed in America. On their limitations in work with English school children, see the criticisms and recommendations of the Consultative Committee of the Board of Education, *Report on Psychological Tests of Educable Capacity* (H.M. Stationery Office, 1924), especially pp. 50–60 and 132. It may be noted that throughout the present report the words temperament and character are used loosely, and almost without difference, to designate individual differences in conative and affective tendencies rather than in cognitive or practical capacities. They thus cover all those personal qualities of mind that do not constitute, or are not pervaded by, intelligence, and includes traits of social conduct as well as of emotion.

to the method of correlation. Estimates were obtained for 32 children. As a control, similar estimates were also secured from a couple of teachers of known ability, who had been personally acquainted with each child for a period of three or more years. The difficulty of obtaining four comparable estimates for the same group inevitably reduced the number to be judged to slender limits; and the probable errors are therefore high. The table below gives the correlations between the judgments (1) of the two observers who were strangers to the children, (2) of the two teachers who knew them well, (3) of the two judges on the one hand and the two teachers on the other, and (4) the last correlation (between judges and teachers) corrected for unreliability.[1]

TABLE XXVII.—*Correlations for Character-Estimates obtained by Ordinary Methods.*

Quality assessed.	Reliability of two observers.	Reliability of two teachers	Correlation between teachers and observers	Correlation between teachers and observers (corrected for unreliability).
Honesty	·19	·51	·14	·45
Conscientiousness ..	·27	·59	·22	·55
Industry	·29	·63	·20	·47
Ambition	·32	·44	·17	·45
Self-respect ..	·42	·56	·31	·64
'Ability to get on with others'	·31	·49	·24	·62
Good humour ..	·39	·54	·27	·59
Averages ..	·31	·54	·22	·54

It will be seen that the agreement of the two experienced observers, judging the children after a single personal interview, is extremely low: in two cases only is the coefficient more than three times the probable error. The agreement between the two teachers who had known the children for several years is far higher; but too much emphasis must not be placed upon this higher correlation, since the teachers concerned had doubtless discussed the children amongst themselves, before they were asked

[1] With coefficients of this order and a group of this size the probable errors vary from $\pm\cdot08$ to $\pm\cdot12$. The corrected coefficients have, of course, no positive significance of their own. They have been calculated merely to show that the data so obtained are not incompatible with the view that, could proper allowance for errors be made, some genuine correlation may underlie the figures obtained.

to furnish estimates. Plainly, if we take the estimates of the teachers as a criterion, the judgments reached by the two observers at the personal interview have little or no practical value. Nevertheless, the calculated coefficients, derived by correcting for the unreliability of the double judgments, are high enough to make it quite conceivable that the method of the personal interview could readily be improved. The question, therefore, arises: how can the recognized principles of psychological examination be applied to the personal interview so as to render its technique sufficiently reliable and exact for use in vocational guidance?

Character Estimates by Psychological Methods.

Some valuable suggestions were gleaned in discussing the chief points of disagreement with those who had been good enough to record their judgments. Two sources of inaccuracy were at once discerned. First, the qualities selected for estimation appeared highly complex and vague: what, asked one of the teachers, is the precise distinction between honesty and conscientiousness, and between conscientiousness and industry? And what is meant by ambition and self-respect? Secondly, the marks allotted for each quality were distributed in a way that could not possibly tally with the truth: one observer, for example, gave full marks for honesty to nearly 80 per cent. of the children. With these and similar defects in view, an effort was made to draw up a simple scheme for judging temperamental qualities. The following were the chief principles employed:

First, it was decided to begin with more elementary qualities of character, qualities which could be expressed in recognized psychological terms, and defined in recognized psychological language. The primary foundations of character are held to be the fundamental instincts and emotions. Hence, the first qualities to be judged in any given individual are the degrees to which these native tendencies are inherited and developed. To this end, the lists of primary emotions enumerated by McDougall and Shand were taken as a basis; and definitions were framed, briefly describing each of these emotional qualities in their various degrees.

It seemed questionable, however, whether these instinctive qualities in their simpler forms are of direct vocational importance. A supplementary list was, therefore, added, containing more complex characteristics, moral, social and temperamental, such as are commonly considered in giving vocational advice. The former qualities are for the most part inborn. The latter are for the most part acquired: they depend upon interests and sentiments, upon personal habits and ethical standards formed afresh by each person for himself during early life. Each of these secondary qualities was again defined in detail as before.

Finally, a rating-scale was framed which should facilitate measurements in numerical terms. The scale itself was not

numerical. For each quality every child was to be marked with one of five letters—A, B, C, D or E—according to the intensity with which the quality seemed developed in him; and the significance of the letters was defined in statistical terms.[1] Plus and minus signs were to be freely used; so that, when the letters were eventually translated into numbers or marks, fifteen grades in all were theoretically distinguishable.

It was originally intended that, according to this schedule, each investigator should rate all the pupils tested at the schools selected for the main research. Unfortunately, this proved too lengthy a task: to complete the entire list for every child would have consumed far more time than either children or investigators could spare. In these schools, therefore, the investigators contented themselves with recording only such outstanding characteristics as were noted in the course of their testing. To assess the reliability and accuracy of such judgments, a separate inquiry was necessary.

In this supplementary inquiry two groups of persons were made the subjects of a complete assessment according to the schedule. The first was a group of thirty, for the most part children about the school-leaving age, who came to the National Institute of Industrial Psychology for vocational guidance or for other reasons. Each of these persons was marked by at least two independent investigators for the character-qualities enumerated. The marks were based partly on observations made in the course of the testing, and partly on a brief interview, lasting from 10 to 30 minutes, specifically intended to elicit his temperamental characteristics.

The second was a smaller group of persons, chiefly young adults, who were assessed in the same way by a number of psychologically trained observers, each of whom was already well acquainted with the persons to be judged. At the same time, one additional observer, to whom the person was not previously known, drew up a parallel assessment based simply upon a single personal interview, such as might be carried out for vocational guidance.

The data so obtained yield a provisional answer to two important questions. First, what is the 'reliability' of impressionistic judgments formed by strangers during a first

[1] The scale is based on the standard deviation as unit; and each of the five grades is defined by percentages. For details see Burt, *Distribution and Relations of Educational Abilities* (P. S. King & Son, 1917), p. 50. Observers unaccustomed to statistical units seem generally to prefer marking qualitative characteristics by letters rather than by numerical marks. From experiments with post-graduate students it would appear that, with non-psychological observers, results more accurate still are obtained if some graphic device is employed, such as that described by Hollingworth *Judging Human Character* (D. Appleton & Co., 1922), pp. 105–7. Here a horizontal line is drawn for each quality to be assessed, divided into five or six segments like a short footrule; and the observer makes a vertical mark on it at a point corresponding to the estimated intensity of the quality.

interview of this sort? How far, that is to say, do two independent observers, using such a method, agree with one another? Secondly, what is the accuracy of such judgments? How far, that is to say, do these impressionistic judgments correspond with the best opinion already available upon each of the persons judged? And, before this latter question can be answered, a third question must be asked: has this criterion itself a sufficiently high reliability to warrant its use as a control?

(a) *The Reliability of the Personal Interview.*

Table XXVIII gives reliability coefficients for the judgments on each of the qualities specified. The data are obtained from the first group of thirty children and young persons, all new to the observers. The coefficients measure the degree of correlation between the two independent observers, marking the same group of applicants after one short personal interview.

TABLE XXVIII.—*Reliability of Estimates of Character Qualites.*

Qualities Estimated.	Coefficient of Reliability.	Probable Error.
I. *Simpler Qualities.*		
1. Submissiveness	·85	±·04
2. Fear	·75	±·06
3. Assertiveness	·74	±·06
4. Sociability	·72	±·06
5. Anger	·71	±·07
6. Tenderness	·68	±·07
7. Cheerfulness	·60	±·09
8. Sorrow	·56	±·09
9. Sex	·51	±·10
10. Disgust	·42	±·11
11. Curiosity	·37	±·12
12. Acquisitiveness	·23	±·13
Average	·60	±·09
II. *Secondary Qualities.*		
1. Self-confidence	·77	±·06
2. Energy	·64	±·08
3. General emotionality	·62	±·08
4. Quickness	·61	±·08
5. Initiative	·57	±·09
6. Co-operation with superiors.. ..	·55	±·09
7. Industry	·54	±·10
8. Honesty	·52	±·10
9. Co-operation with equals	·50	±·10
10. Co-operation with inferiors	·49	±·10
11. Punctuality	·44	±·11
12. Reliability	·36	±·12
Average	·55	±·09

All the coefficients but one are more than three times the probable error.[1] Their average is ·57. The most striking thing, however, about the figures is the extraordinary range of divergence: for one of the qualities so marked the reliability is as high as ·85, a coefficient that would be thought extremely satisfactory even for a well-standardized test; for another quality it sinks to ·23, a figure which has little or no significance as compared with the probable error. Plainly, therefore, so far as these results can be trusted, it must be far easier to reach consistent estimates for some qualities than for others; and it becomes of prime importance to discover what particular qualities can be so assessed with the greatest reliability, and which of them cannot be readily assessed in this manner at all.

(i) Generally speaking, it would seem that the simpler or primary qualities can be estimated with greater reliability than those that have been termed secondary or complex. For this difference there are many obvious reasons; perhaps the most essential is that the names describing these qualities have now among psychologists a fairly well-recognized connotation.[2]

(ii) Among the primary qualities, those that are distinctly emotional are by far the most easy to assess. Acquisitiveness and curiosity, for example, instincts which have little or no accompanying emotion, yield estimates that are the least reliable in the whole list. The reason is evident: emotion is easily evoked in a personal interview, and, with younger people, at any rate, betrays itself directly by change of voice or facial expression.

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[1] For one or two character-qualities estimates were not made for the entire group; hence the probable errors are at times slightly higher than might be expected with a group of this size.

[2] In the present report the actual definition given to each of the terms in our list, whether simple or complex, is of no great moment; and here, therefore, need not be specified. It is hoped, after fuller investigations have been made, to draw up an improved list, with definitions, not only for every quality, but for each of the five distinguishable grades which every quality presents. It may be added that we are fully aware that what have been called simple innate qualities are only relatively simple and but partly innate. Recent writers have made much of the fact that the so-called sex instinct is an aggregate of numerous partial tendencies. The same might be said of almost every other instinct. Fear, for example, as McDougall himself recognizes, has two forms corresponding to the instinct of flight and the instinct of self-concealment; and further, there are distinct individual differences, according as fear is excited chiefly by persons (as in the shy person) or by inanimate stimuli like noises or dangerous situations (as in the coward). The actual manifestations, too, of inherited instinct or emotions in the older child are inevitably modified by past experience, by habits, sentiments, and complexes, and by self control.

(iii) Further, among these emotional qualities, those that are excited by human relations, such as naturally arise in all social situations, are the easiest to estimate of all. They have, in fact, high reliability coefficients rising usually to ·70 or over. Whether the examinee's manner is sociable or timid, cheerful or bad-tempered, assertive or submissive, these are points which, in the same office and on the same day, are bound to impress different interviewers in much the same manner. It does not, of course, follow that where the candidate's momentary mood or disposition has prompted an identical estimate with independent judges, their judgments are, therefore, a true estimate of his permanent character. But it is clear that, unless they so agree, their judgments can have little worth.

(iv) Among what have been termed secondary or complex qualities, those that spontaneously emerge in test-performances appear to have the highest reliability—quickness, energy, initiative, self-confidence, and perhaps, industry. These have all been classed with secondary qualities, on the ground that they are not included in the customary lists of primary emotions, but appear to be complex derivatives of several instinctive tendencies. Nevertheless, it is possible that nearly every one may be in fact an elementary quality of the mind. Quickness, for example, and energy, and general emotionality, have each been put forward by one writer or another as being in themselves based on some specific central factor. Among those that are undoubtedly complex, the differences in reliability are perhaps[1] traceable to varying degrees in which they are actually called into play: the examinee's co-operativeness with superiors is naturally assessed more consistently than his degree of co-operation with equals or inferiors, since in most cases the candidate naturally looks upon his interviewer as his superior for the moment.

(v) Where the quality cannot be directly elicited in the course of an ordinary interview, and where the estimates have therefore to be based on indirect inference, there the reliability of the judgment is low—for example, punctuality.

(vi) Where the quality to be assessed is a moral quality rather than a temperamental one, and where an extreme manifestation of it (or of its opposite) constitutes an anti-social, unmannerly, or even criminal trait, there, naturally enough, its expression may be successfully concealed, and an estimate of its amount can seldom be relied upon. Thus judgments for honesty, reliability, and sex, yield decidedly low coefficients; and those for inquisitiveness and acquisitiveness yield coefficients poorer still.

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[1] In the list of coefficients here given the differences are too small (as compared with the size of the probable errors) to possess much significance.

(b) *Reliability of Character-Estimates by psychologically trained Acquaintances.*

To prove that the estimates of two observers agree with each other does not prove that those estimates necessarily agree with the truth. We must, therefore, seek some standard or control by which to check them. The obvious procedure is to compare estimates reached after one short interview with estimates supplied by those who have known the examinee over a long period of years, and are themselves competent psychological observers. In the case of chance applicants for vocational guidance, a control-rating of this sort is almost impossible to procure. In the case of children still at school, opinions can generally be got from teachers; but these opinions are themselves often vitiated by the fact that they are seldom based upon sound psychological principles or expressed in unambiguous terms. The only course open to us, therefore, was to make a special study of a small group of persons who were already intimately known to several psychological observers, and who would be willing to submit themselves to a special interview by a stranger. These were the persons who formed our second group.

Let us be sure, to begin with, that these psychological observers agree amongst themselves Judgments from at least four observers were obtained for six persons. For each of the six the judgments were then averaged to form two series of composite assessments. Thus, for Mr. C.D., all the qualities judged were arranged in order of intensity on the basis of the averaged judgments of Observer No. 1 and Observer No. 2; and, for the same man, the same qualities were re-arranged a second time on the basis of the judgments of Observers No. 3 and No. 4: the correlation between these two composite arrangements was then computed. The coefficients so obtained for each of the six persons judged are shown in the first two columns of Table XXIX.. Here what have been correlated are not the assessments for the same character-quality among a group of different individuals, but the assessments for a set of different character-qualities drawn up for the same examinee. At first sight, this might seem the simpler method, and higher coefficients might naturally be expected: to pick out the strongest and the weakest points in a given individual should be an easier task than to compare the strength and weakness of a given characteristic in a number of persons interviewed on different occasions. But suppose the man assessed has no marked points of strength or weakness; suppose all his qualities are evenly developed; then, plainly, the coefficient may be reduced almost to zero, for there will be hardly any variations to correlate. It is essential, therefore, that each examinee selected should have different qualities differently developed.

TABLE XXIX.—*Reliability and Accuracy of Character-Estimates by Psychological Observers.*

Subject.	Correlation between Judgments by Acquaintances (Reliability).		Correlation between Judgments by Acquaintances and Judgments by Subject.		Correlation between Judgments by Unacquainted Interviewer and Judgments by Acquaintances (including the Subject himself.)	
	Primary Qualities.	Secondary Qualities	Primary Qualities.	Secondary Qualities.	Primary Qualities.	Secondary Qualities.
A.B. ..	·86	·61	·93	·69	·70	·58
C.D. ..	·64	·77	·52	·67	·63	·55
E.F. ..	·81	·67	·47	·46	·55	·43
G.H. ..	·63	·78	·66	·64	·47	·45
I.J. ..	·78	·62	·63	·56	·59	·40
K.L. ..	·76	·65	·60	·61	·57	·38
Average	·75	·68	·63	·60	·58	·46

From the table here given it will at once be seen that the reliability is high. It averages ·75 for the primary qualities, and ·68 for the secondary. Once more, it will be seen that, even with adults, the primary qualities prove, as a rule, easier to estimate than the secondary. The few exceptions are readily explained: here the two persons judged were known to the individuals judging, not so much from their private life, as from semi-official relations—in the office, the college, or the school. Apart from these special differences, the unusual magnitude of a few reliability coefficients is due to the fact that there were more observers judging, and that the observers were better acquainted with the person to be judged. A.B., for example, was assessed by six different judges, all intimate friends with psychological training or experience: each had known him for periods varying from four to twenty years, working together in the same office, living together in the same home, or travelling together abroad. For his primary or emotional qualities the estimates from the first three judges correlate with the estimates from the last three to the extent of ·86, a figure quite as high as the reliability coefficients usually obtained for the criteria used to check the ordinary kinds of mental tests. The examinee himself drew up an independent estimate of his own character; and his estimate for emotional qualities correlates with the estimates of the six judges to the extent of ·93. It is clear, therefore, that, at any rate for the emotional qualities of this particular subject, we have a most reliable criterion: it is difficult to conceive anything more trustworthy. Judged by this criterion, what is the accuracy of a stranger's estimates?

(c) *Accuracy of Character-estimates by Psychologically-trained Interviewers.*

The examinee just mentioned submitted himself for interview to a psychological visitor from abroad, a man to whom he was unknown except by name, but who had had considerable experience in temperamental ratings and who volunteered to apply the methods and the rating-scale described. The rapid judgment formed after one short interview of forty-five minutes correlated with the opinion of the seven independent observers (including the examinee himself) to the extent of ·70 for primary qualities and ·58 for secondary.

This, perhaps, is an exceptionally favourable instance by which to judge such estimates. Yet it is useful to know what is the highest measure of success that may be expected to attend such methods. The five other subjects were also interviewed by a psychologist previously unacquainted with them; and his judgments were compared with the averaged judgments of their friends. The whole series of correlations is shown in the last two columns of Table XXIX. The average accuracy obtained at such interviews proves to be ·58 for primary qualities and ·46 for secondary qualities.

At first sight these figures are less encouraging than could be wished. Contrasted with the correlations obtained from intelligence tests, they are undoubtedly low. With the correlations obtained from existing tests of temperament and character, however, they compare quite favourably. With tests of the latter type, applied to English children and adults, the reliability varies from ·40 to ·75; and the correlation with independent estimates of the qualities tested from ·35 to ·65.[1] Thus, compared with direct tests of similar qualities, the results obtained at a personal interview are distinctly superior for the primary emotional qualities, but little if at all superior for the secondary or moral qualities.

The smaller coefficients shown in the table are by no means uninstructive. The psychologist who interviewed E.F. and G.H. thought it wiser to refrain from putting the usual searching questions into the candidate's emotional history, and from subjecting the candidate to any experimental test. Subjects I.J. and K.L. were, indeed, tested and interviewed as though they had applied for vocational guidance—the tests being of the type above described, namely, tests of intelligence educational attainments, and manual and constructive skill; but these two subjects were themselves psychological students, possessing sufficient motive, knowledge, and intelligence to make a more favourable impression on their interviewer than their moral qualities really warranted.

[1] Burt, *The Young Delinquent*, p. 412.

It may be noted that the foregoing data prove quite clearly that the power to judge character is itself a variable characteristic. Indeed, the procedure here employed might even be easily adapted to obtain a measure of the interviewer's own reliability and accuracy as a judge of character. It is significant that, throughout the judgments we have examined, the women interviewers showed more reliability and accuracy than the men, and the practised psychologists more than the casual observer. Practice itself undoubtedly improves the power to judge another: with the same two observers, judging applicants for vocational guidance, the average reliability was, with the first six candidates, only ·52, and rose, with the last six, to ·69.

One important conclusion emerges from all these observations. Judgments on well-defined elementary traits, particularly those of an emotional or temperamental quality, like timidity, sociability, and cheerfulness, are, as a rule, far more consistent and far more accurate than judgments on the more complex moral qualities, like honesty, industry, and reliability. Strangely enough, in ordinary interviews, it is the latter type of trait on which the interviewer most commonly reports, and of which probably he believes himself to be a truer judge; the latter, too, are the traits which, at first sight, seem the most relevant in making vocational recommendations. But, if the foregoing inquiry can be trusted, judgments of moral quality are extremely fallible; and, for these in particular, tests, when more carefully developed, will probably prove far more valuable than impressions gleaned at an interview. Nothing, however, can form a substitute for precise and detailed reports secured from parents, teachers, or welfare-workers, who know the child and his history. For the rest, moral qualities are in the main acquired, and emotional qualities largely inborn. Hence, there is every reason to suppose that moral qualities are the more changeable, and emotional qualities the more permanent.

We strongly recommend, therefore, that, in future inquiries, the interviewer's attention be directed primarily to the assessment of emotional qualities, and that, pending the development of moral tests, the assessment of moral qualities be based chiefly on confidential reports from teachers and parents. It would appear, too, that moral and social qualities are in urgent need of the same exact analysis and the same precise definition that is available in the case of emotional qualities.

Temperamental Assessments in Vocational Guidance.[1]

What is the bearing of this incidental study upon the vocational guidance of school children? With one or two exceptions, the strangers interviewing the candidates as above described were

[1] In writing this section Miss Spielman and Professor Burt wish to express their indebtedness to Miss Smith, Miss Gaw, and Mrs. Ramsey: data supplied by them have been freely incorporated in the paragraphs that follow.

investigators of the National Institute of Industrial Psychology engaged upon the main research which forms the subject of this report. Hence the correlations for reliability given in Table XXVIII, and the correlations for accuracy given in Table XXIX, may be taken as a fair index of the reliability and accuracy of these investigators' judgments as made in the course of the main inquiry.

In passing our final judgment on the character of each individual child, we gained much assistance from the histories and the comments supplied by teachers and parents whom we visited.[1] Often, indeed, it was found that the information was elicited with greater detail, readiness and exactitude when the points were approached indirectly rather than directly. Gradually we found ourselves working towards a systematic schedule of productive questions which could be usefully put in almost every case. What kind of games the child habitually played, what were his hobbies, his favourite amusements, and his preferences in general reading, whether he played alone or with other children, whether he took the lead himself or submissively followed the lead that was given by others, whether he was popular among his school-fellows, whether he assumed a responsibility for the younger children at home, whether he was a prefect or a monitor in his class, and whether he was a member of a club or of a troop of scouts—these and similar inquiries often brought the most illuminating replies about the child's temperamental characteristics.

The tests described in earlier sections proved rich in opportunities for observing moral and emotional qualities indirectly. Time after time we have been asked how the psychologist could allow for the nervousness that most children feel when summoned for a special interview. The easiest way to overcome such nervousness is to give the child something to do; so soon as he becomes interested in a practical test, like the form-boards or the picture-puzzles, he begins to reveal his normal qualities and to behave in his natural or habitual manner. Indeed, the examiner who has recourse to such tests finds the problem of nervousness vanish almost entirely; and gains a far better opportunity for judging the child's real character than if he simply questions him directly, after the manner of the ordinary interviewer.

In the performance tests in particular, several important points could be noted with ease again and again.[2] (1) The child's general speed of movement was nearly always suggestive; it seemed possible to infer whether in the ordinary situations of industrial life he would be quick or slow, calm or excited, impulsive

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[1] See preceding footnote.

[2] In an earlier paper ("A Study of Performance Tests," *Brit. Journ. Psych.*, *XV*, 1925, p. 389) Miss Gaw has pointed out how temperamental qualities—such as impulsiveness, initiative, dominance of purpose—are frequently brought out by non-verbal tests like the Porteus Maze-tests and the Kelly test of constructive ability.

or deliberate, likely to do his best when left to himself or needing a little external pressure to bring out his maximum efficiency. (2) Equally instructive was the effect of success and failure. One child would be almost indifferent whether he accomplished the tests or not; another would become worried or even exasperated by the smallest difficulty; and a third would be so anxious and so over-scrupulous to avoid any mistake, that he bungled all but the most habitual and mechanical reactions. There was, too, a marked difference between the children who would persevere in spite of partial failure, and those who were inclined to give up at every obstacle, even in tasks appealing to their deepest interests. (3) A third point that could be noted was the presence or absence of self-criticism: the self-complacent child would be satisfied with any result however poor; the over-confident child would take up the hardest problem with the same eagerness as the easiest; the diffident child would search again and again for possible errors when in point of fact his work was complete and irreproachable.

Among the tests of a non-performance type, the questionnaire on creative imagination was one of the most illuminating. The fantasies and self-told stories which many of the children described often threw much light upon the moral and emotional peculiarities of the writers. One small delicate girl, outwardly shy and retiring, wrote that, in the tales that she told herself at night, she always imagined herself to be Hercules. A boy, not otherwise known to be dishonest, wrote, in answer to the question—"What is the cleverest thing you ever invented?"—"I have thought of a way of getting gas without putting money in the meter, by a hole in the pipe and fixing another pipe in it. It won't be seen." Contrast, too, the following answers given respectively by a neat, quiet and conscientious girl, and an untidy boy of selfish disposition. The question was—"Have you ever imagined a room just as you would like it best? What was it like?" The first wrote: "With all the brass polished, the beds with all white sheets and blankets, a beautiful clean window, and everything looking bright." The second wrote: "Cushions, chairs, arm-chairs, electric lights, bathing room, tennis ground, servants, and wine with dinner, waitresses, motor-car, mats and carpets of bear skin and polar skin."

Quite apart, however, from any special tests and questions, the child's first response to the several strangers who interviewed and investigated him was nearly always significant. Each investigator was asked to observe whether the child's manner was easy or not, whether he was embarrassed, quiet, or shy, whether he was openly sensitive to praise and blame, whether he seemed conscious or unconscious of being observed, whether he was taciturn, communicative, or exceptionally garrulous, and whether he proved readily distracted by the persons around him. It is, of course, impossible to be sure that the child's behaviour at such an interview is a typical reaction, a response habitual with him in such situations. Nevertheless, as each was seen by four different investigators on several occasions, the chances of gross

misinterpretation were greatly diminished. Sometimes, indeed, it happened that a child seemed nervous with some tests and not with others, excitable on one day and apathetic the next, awkward with this investigator and natural with the rest; and these very differences themselves, when subsequently discussed and analyzed, almost always proved instructive. On the whole, however—as, indeed, the correlations cited above might lead us to suspect—there was a remarkable similarity in the child's behaviour during the different interviews, and a high measure of agreement among the four investigators concerned.

How the qualities thus assessed bear upon vocational guidance must be obvious to all who have had experience of the needs of industrial life. Perhaps the most important point is to decide how well the examinee is likely to get on with other people. What may be called the personal emotions—the emotions aroused by the presence of other human beings—happen, as we have just implied, to be among the easiest to gauge. Nor is it difficult to pick out among the examinees two main contrasted groups; first, those who are interested in persons and usually work best in the company of others, and, secondly, those who are not at their best in human relations, and are less interested in other persons than in inanimate objects or things. When intellectual as well as temperamental qualities are taken into account, each of these two main groups may be cross-divided into a pair of sub-groups. There are those who like dealing with persons directly, and those who prefer to deal with them indirectly and at a distance through correspondence and written records or reports. Similarly, there are those who like dealing with things at first hand, and those who prefer to deal with them indirectly through descriptions or calculations on paper. If we class together the second sub-group from either pair, we have a threefold classification of the greatest value in vocational recommendations. We have to ask, of each examinee, whether he is likely to deal best with persons, with things, or with papers. An examinee belonging to the first of these three classes is obviously suited to employments that bring him into active touch with other human beings; he is fitted for such vocations as that of a teacher, salesman, or commercial traveller. An examinee in the second class is more suited to impersonal work, such as that of the chemist, the mechanic, or the engineer. An examinee in the third class is best suited to what may broadly be called office work—the handling of reports, correspondence, and files.

Other temperamental qualities may be of importance in choosing or rejecting occupations more specific. The youth marked low for honesty is hardly to be trusted in a bank, a post-office, or a pay-box, where he may have to handle tempting sums of money. The youth marked low for industry or reliability is unlikely to give satisfaction in a post which calls for steady conscientious work with little or no supervision. The timid, nervous lad is not fit for any occupation involving daily risks or needing prompt decisions in emergency, like that of the airman, the sailor, or the chauffeur. The shy, submissive person

will probably fail hopelessly in posts requiring leadership, disciplinary force, or self-reliant independent action. The boy marked low for assertiveness will hardly do well in a business requiring confidence and push. The girl marked low for tenderness is unsuited to be a teacher or a nurse. The talkative girl may be miserable in a room to herself, a nuisance in a workroom with other people, but a marked success as a saleswoman. The bad-tempered person will be a source of trouble in a factory or workshop where he must submit to rough manners and bluff criticism. The unstable person, always wishful for change and excitement, will not take well to a monotonous and sedentary life or to a daily round of repetition work. On the other hand, the nervous person with dreamy habits will probably be happier and more efficient if she can be left to herself with nothing but the same mechanical routine as day follows day.

These, then are the more important temperamental qualities which it was found essential to assess; and these are the ways in which such assessments determined the recommendations given. Although, in the three schools where the main investigation was carried on, it was not found possible to assess every child systematically for each of the qualities enumerated, nevertheless, outstanding characteristics were noted under these heads; and the notes were found to be of the greatest value in deciding at the end of the inquiries what particular career should be selected for each individual child.

VII. Home Conditions.

By Lettice Ramsey.

General Description of the Homes.

The homes of the children tested were specially visited; and, in accordance with the general scheme drawn up by Dr. Burt, the following particulars, where possible, were obtained.[1]

1. *Home conditions.*—Names, ages and occupations of all members of the family. Number, size, and condition of rooms occupied. Rent and total family income. Special conditions such as death or desertion of parents or the presence of foster-parents.

2. *Family history.*—Health, intelligence and character of the various members of family.

3. *Personal history*, including a record of the health past and present of the child himself, schools attended, and changes of dwelling place.

4. *Out-of-school behaviour*, including parent's report upon child with particular reference to his intelligence, temperament, interests, special abilities, hobbies and amusements.

5. *Intended occupation.*

[1] As will be seen from the tables, it proved impossible to gain full particulars for every one of the hundred cases selected for special study. No satisfactory data were available for the prosperity of the home in the case of four children, nor for the intelligence of the mother in the case of twenty-one.

In nearly every case it was possible to obtain fairly full information on all the above points; and in no case did the parents seem to resent the inquiry. Many were pleased at having so much interest taken in their children; and thought the scheme for vocational guidance "an excellent idea."

The district from which the schools were selected for the investigation had been chosen as representing a fairly average sample of the population of London. The houses visited were, on the whole, neither very poor nor yet very prosperous. After all the visits had been made the homes were grouped into four classes—(i). Superior, (ii). Good, (iii). Moderate, and (iv). Poor.[1]

This classification was based simply upon a personal estimate of the "general prosperity" of each home. The number of persons per room, the condition both as regards cleanliness and comfort, and the class of house in which the rooms were situated, together with the probable or known income of the family, were the chief factors taken into account. An effort was made not to be biased by the intelligence or affability of the parent interviewed. After the classification based upon "general prosperity" had been made there were found to be—

14 "superior" homes.
34 "good" homes,
32 "moderate" homes,
16 "poor" homes.

These were very evenly distributed among the three schools where the tests were carried out, there being no significant difference between the general prosperity of the families from which the several schools were recruited.

In the superior homes the average number of persons to each room was 1·3; in the good and moderate homes, 2·0; and in the poor homes, 2·7.

The average number of children was 3·9 in the superior families, 4·6 in the good and moderate, and 5·9 in the poor.

The chief occupations of the parents were as follows:—

TABLE XXX.—*Occupations of Parents.*

FATHER'S OCCUPATION.		MOTHER'S OCCUPATION.	
Railway (porters or carters)	15	Charing	12
Engineering	11	Cleaning offices, etc.	7
Building and decorating	10	Shop	3
Borough Council	7	Tailoring	1
Shopkeepers	6	Waitress	1
Clerical	5	Cook	1
Policemen	3	Barmaid	1
Van driver	3	Confectionery	1
Printing	3	At home	61
French polishing	3	Dead	9
Piano making	2	Not ascertained	3
Cabinet making	2		
Hotel porter	2		
Invalid	3		
Dead	9		
Miscellaneous jobs	13		
Not ascertained	3		
	100		100

[1] The classes may be taken as corresponding approximately to Charles Booth's classes F, E, C, and D respectively (*Life and Labour in London*, I, pp. 33 *et seq.*).

The following examples will make clear the type of children we were dealing with :—

Superior Home.—The father is a builder and decorator owning his own business which brings in a steady income of about £7 per week. The mother is an intelligent woman with a pleasant disposition. She keeps the five well-furnished rooms very neat and comfortable. There are four children; the eldest son drives a motor van and earns £3 a week ; the eldest daughter earns 25s. a week as assistant dressmaker in a good firm. Annie, the girl tested, is just about to leave school. She is much above the average in intelligence: (mental ratio, 128). Her tastes and abilities are all on the literary side, prompting her to occupy her spare time in reading and writing stories. She wishes to take up secretarial work ; and will probably get a good post when she is older. The youngest boy is still at school.

This family all get on well together and seem very happy. Annie is not forced to earn a large wage immediately; and she has been advised to train in secretarial work.

Good home.—The father is a goods checker on the railway, earning £4 per week. He has five children—two girls working at local factories, Jennie, the girl tested, and two younger children still at school. There are three well-kept rooms. The mother is intelligent, pleasant and friendly.

Jennie is rather slow and retiring, and has no "push." She is fond of reading and sewing, but not very good at either. Her general intelligence is poor (mental ratio, 81). She was recommended for factory work; and a place has been secured for her at the factory with her sister. She now says that she is "glad she didn't try to become a dressmaker."

Moderate home.—The father drives a brewer's dray, earning £2 14s. 0*d*. Of the five children, one boy is in the Navy and contributes 9s. to his home, another is apprenticed at 10*s*. to a scientific instrument maker, three are still at school. Three rooms well kept: rent, 10*s*. The mother is very intelligent, emotional and rather irritable. Maurice, the boy tested, is intelligent (mental ratio, 112) ; he is very energetic and has plenty of initiative. He wants to be a telegraphist ; and his quick reactions in the tests of manual dexterity made this an excellent calling for him to take up.

Poor home.—Father, carriage washer on the railway, wages 35*s*. per week. Five children all under 14. Jerry, the boy tested is the eldest. Two rooms; very untidy, dirty and poorly furnished, in a poor class house : rent, 8*s*. 6*d*. The three youngest children are not very strong. Mother moderately intelligent, cheerful and pleasant. She says Jerry is intelligent, likes an outdoor life. He is not clever with his hands and of no use in the

house. His mental ratio is only 84. He wants to go on the railway; but his parents prefer an office. It is clear that he could never make a competent clerk, and was recommended to become a junior porter. He is now very satisfied with his work, and is said to be doing well.

Relations between Home Conditions and Intelligence of Mothers and Children.

A. Comparison of Mother's Intelligence with Prosperity of Home.

An attempt was made to grade the intelligence of the mother—the parent most often interviewed. Only a rough estimate could be made during an interview lasting from 20 to 40 minutes; and of course this estimate was entirely a personal one based upon the judgment of one investigator only. Notes were made immediately on leaving the house, and the mother described as (1) Very Intelligent (A+); (2) Intelligent (A); (3) Moderately Intelligent (B); or (4) Unintelligent (C).[1]

When all the visits had been completed a comparison was attempted between these four groups and the prosperity of the homes from which they came. The results are summarized in Table XXXI. If the homes are arranged in order of prosperity and the mothers in order of intelligence, the correlation between the two proves to be ·31 (probable error, ±·07).

Table XXXI.—*Intelligence of Parent compared with Prosperity of Home.*

Class of Home.	Number of Mothers.	Number of Mothers in each Class showing the Grade of Intelligence specified.			
		A+.	A.	B.	C.
Superior ..	13	3	4	3	3
Good ..	27	3	16	8	0
Moderate ..	25	1	8	12	4
Poor ..	14	0	2	9	3

[1] This classification may be taken as roughly corresponding to the classification given above in Table IV. A corresponds to Class IV (average mental ratio, 107); B to Class V (average mental ratio, 92); and C to Class VI (average mental ratio, 79). The few cases falling into Classes I, II, and III, are grouped together as A+ (average mental ratio, 124).

B. *Comparison of Child's Intelligence with Prosperity of Home.*

The average Mental Ratio of the children from each of the four classes of homes was found to be as follows :—

TABLE XXXII.—*Intelligence of Child compared with Prosperity of Home.*

Class of Home.	Number of Children.	Average Mental Ratio (Binet tests).
Superior.. ..	14	109·5
Good	34	98·8
Moderate ..	32	91·5
Poor	16	89·3

The correlation between the intelligence of the children and the prosperity of the home is ·43 (probable error, ±·06). The group is too small, and the classification too rough, to permit of much importance being attached to this figure. The result obtained, however, is in close agreement with those published in a former investigation undertaken by the Medical Research Council[1] : and points to a distinct but not very high correlation between home conditions and the intelligence of the children.

Note.—For the purposes of vocational guidance, the point that should chiefly be emphasized is not the presence of the correlation, but its comparatively low degree. In terms of the mental ratio, the average intelligence of the boys and girls coming from superior homes is only about 20 per cent. above that of the children coming from the poorest. On the other hand, within any one of the four economic groups, the intelligence of the individual boys and girls varies considerably. Accordingly, it would be extremely unwise to recommend a child to take up a superior occupation, simply because he comes from a superior home, and looks well clad, well groomed, and well cared for, and has the manners of the well-to-do. And it would be still more wasteful to assume that because a child's home conditions are poor that his mental efficiency is therefore fit for nothing but occupations of a lower grade. In the long run it may, indeed, be true that children bright enough to win scholarships are far less numerous in the families of the poor than in those of the better classes. Yet, in giving advice to individual cases, the essential fact to realize is this : the range of individual variation is so wide, that from time to time children of high intelligence may be forthcoming even from the poorest homes and in the poorest neighbourhoods ; and, hence, a special watch is needed, and perhaps a special type of test should be applied, to discover these unexpected cases.

C. *Relation between Intelligence of Mothers and Children.*

The intelligence of the mothers, based, as before, upon the personal interview, was compared with the intelligence of their children as determined by the Binet tests. The results are summarized in Table XXXIII. So far as they go, they are consistent

[1] Isserlis and Wood (1923) :—The Correlation between Home Conditions and the Intelligence of School Children (*M.R.C. Spec. Rep. Ser.*, No. 74, *pp* 17–18.).

with those obtained by previous investigators, which tend to indicate that a child's intelligence is correlated somewhat more closely with the intelligence of his parents than with the material prosperity of his home. The correlations given by our data are ·51 and ·43 respectively: but, with a group so small, the difference is barely significant when compared with the probable error (±·06).

TABLE XXXIII.—*Intelligence of Mothers compared with Intelligence of their Children.*

Intelligence of Mothers.	Number of Children.[1]	Average Mental Ratio.
A+	7	110·6
A	30	104·0
B	32	93·9
C	10	85·1

[1] In no case was there more than one child tested from each home.

BEARING OF THE HOME INQUIRIES UPON VOCATIONAL GUIDANCE.

At first sight it might be thought that these inquiries had but little relation to the problem of the child's fitness for a particular employment, and that the few particulars required could be got by inviting the mother to the school for a short interview. It proved, however, that the additional knowledge disclosed during special visits to the homes was of great assistance.

To begin with, many of the child's individual traits cannot be discovered, or at least cannot be understood, except in the light of full information about his family history and his out-of-school surroundings. With exceptional children of every type, bright or dull, difficult or troublesome, the question constantly arises—how far are their special characteristics inborn and therefore permanent, and how far do they spring from the advantages or limitations under which each child is living? This question, as a rule, can only be answered after a special study of his life at home.

The parent's description of the child's interests, hobbies, and every-day behaviour, throws light, not only on his non-scholastic aptitudes, but also on the more elusive problems of temperament. Further, it is always essential to take into account the parents' own desires as to the future occupation of their child, and to learn what responsibilities he may have to shoulder at home, what encouragement he is likely to receive, how far his private ambitions will be fostered, and to what extent his further training and education may be assisted. One question has always to be answered. How far do the family conditions make it imperative for the child to find remunerative employment, either

immediately or in the near future? With these special problems in view, it is essential to know, not merely the size of the family and the amount of the income, but also the regularity of the income, and the likelihood of its increasing or decreasing in the near future owing to the age of the parents or of the older children.

It was found that many parents wished their children to follow the same trade as themselves. In such circumstances they could often give the child material assistance in his training, or suggest for him an immediate vacancy of the kind required. And generally, whenever the child's parents or friends had some definite situation in view for him, it seemed wiser not to urge a different alternative, unless it had become clear beyond dispute that the child's abilities would not be equal to the work, or that his temperamental inclinations were hopelessly opposed to it.

Often, indeed, the social character of the child and of his home brings difficulties of its own. In a few instances, among the poorer districts visited, the child's family seemed likely to prove not a help but a handicap, and more than once the main recommendation that we had to make in the child's best interests was this—that, whatever engagement was ultimately found for him, it was desirable that he (or she) should reside away from home—placed, perhaps in domestic service, or sent to a firm which required its employees to 'live in.' Sometimes his very home address may be a drawback. In certain firms and offices we were informed that there seemed to be an element of snobbishness or exclusiveness among the employees; in one place, a child from a cultured home, placed among rougher companions, might find himself despised for seeming supercilious or super-refined; in another, a child from a home less cultured might be boycotted for his accent, manner, or style of dress. Not for a moment was it our intention to preserve or foster a spirit of class distinction; we sought simply to consider to what extent the actual facts of the child's home life might penalize or favour him. Always it was our intention to give the boy with special intelligence or special talent a fair chance to better himself, no matter how poor and abject his home conditions might seem; and, as a rule, these considerations altered, not so much the type of employment we were recommending, as the type of firm to which he might be advised to go.

Above all, it seems desirable that the ultimate decision should be formed by the parents or by the child himself, not by some outside expert or official presenting them with a ready-made decision. Generally speaking, the proper policy for the vocational adviser is not to confront the family with a single pre-arranged recommendation, but rather to say to them: "these are the special qualifications or disqualifications of your child; these are the types of employment which seem open to him; they will impose such and such requirements, and will have such and such advantages; it is for you to make the final choice."

VIII. Physical Conditions.

By Lettice Ramsey.

It was not thought possible or desirable in this preliminary research to arrange for a special medical examination. Before deciding to what extent such an examination is necessary for sound vocational advice, it seemed wiser first of all to discover how far the points already ascertained might be of genuine help.[1]

We were, in every case, allowed access to the medical cards recording the results of the medical inspections previously carried out at the school. These data were further supplemented by inquiries from the teacher, and (most helpful of all) by inquiries from the home. The results were always duly weighed before a final recommendation was put forward.

A general rating, or averaged assessment for the child's health as a whole, would be of great assistance in vocational guidance.[2] Different callings make very different demands on sheer physical stamina—on the power to work hard and continuously, resisting fatigue and petty ailments; and this is a quality which has little relation to special illnesses, deformities, or defects. The marks for 'nutrition' entered on the card, together with the child's height and weight, were in this respect often suggestive; but the impression formed by the interviewer of the child's physique, vitality, and general bodily appearance, together with the accounts of the parents and school teachers, was often for our purpose far more helpful.

For the rest, it appeared that physical conditions were far less important than mental conditions. Only in exceptional cases was it found that such data bore directly upon the choice of employment; and then the inferences made were of a negative rather than of a positive type.

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[1] Should any more extensive research be organized, we now feel disposed to recommend the introduction of a special physical and medical examination. It is a common experience that, in dealing with defective and delinquent children, the ordinary routine medical inspection proves often to be too brief and inadequate to bring to light even the relevant physical peculiarities: a special examination by a medical specialist is nearly always to be desired. So too for studies in vocational guidance: a special examination by a medical officer, familiar with the particular points requiring to be settled for such a purpose should, if possible, be arranged, if only to see whether such special examinations are likely to produce any new or important information. As we ourselves can testify, the interest of the school medical department in such work as this is so great and genuine that it should not be difficult to enlist the co-operation of the school doctors as well as the school teachers.

[2] An interesting side-issue for research would be an investigation by purely statistical methods into the existence of a general health factor analogous to the general intellectual factor known as intelligence. The validity of such a conception—or at least of such a mode of averaging the specific assessments into a final grading—would be of great help to the vocational adviser.

The child's height is always recorded upon the school cards; and has sometimes to be taken into account in advising certain employments. For a number of callings—that of a soldier or policeman, for example, or lads entering the post office—a minimum height is essential. In other cases, a boy below the normal height, particularly if he is active as well as small, can be usefully recommended for such situations as that of a page, where a low stature rather than a tall one is preferred. In certain occupations, too,—such as those of a waitress, a salesman, a mannequin, or a parlour-maid in a superior household—good looks, a good bodily carriage, and an attractive appearance generally, are often an important asset.

The weight of the child as compared with his height, and the general notes made as to his muscular condition and general nourishment, are often suggestive; and might perhaps sometimes be supplemented to advantage by dynamometric measurements of muscular strength, and by specific tests or observations which might seem of small significance from the standpoint of health, but would prove of great importance in particular trades or industries. Many occupations involve muscular strain, strength of particular limbs or muscle-groups, exposure to bad weather, standing for long hours, lifting heavy weights, and the like. Children with weak hearts, flabby musculature, or special conditions such as hernia, are obviously to be warned against heavy labour. Often, too, the child's so-called diathesis, or the parents' account of his past ailments, may negative any occupation involving excessive cold, heat, or damp.

Many forms of weakness make prolonged hours of sitting wholly inadvisable. Respiratory troubles generally are unfavourable to work indoors, particularly work in a dusty or ill-ventilated atmosphere. The so-called pre-tubercular child—to take a special instance that came more than once under our attention—was, where possible, persuaded to take up work in the open air—at a bookstall (for example) or as a checker on a railway.

Sensory defects were also noted. A child who was slightly deaf could not be recommended to take up any occupation bringing him into constant contact with other persons, particularly with strangers. The child suffering from visual defect, particularly hypermetropia or astigmatism, was advised not to take up occupations involving eye-strain, such as needlework, even where the child desired it. Children with speech defects were similarly urged to take up impersonal rather than personal occupations, and were warned not to accept such a position as that of shop-assistant. Quite minor conditions may at times be of the utmost importance. It is essential, for example, that the laundry girl should have strong arms, and that the girl who takes up needlework with delicate materials should have dry hands.

Sometimes, it is true, we felt that in the child's best interests some of these contra-indications would have to be ignored. We have known cases of children with defective vision who have

been told never to take up close work requiring a constant use of the eyes, who, nevertheless, defied the warning, and have seemed to suffer not the slightest harm in consequence. We recollect one case where in theory the child should have been cautioned not to take up any occupation requiring more than average intelligence, or involving muscular strain, nervous strain, exposure out-of-doors, or an ill-ventilated atmosphere indoors. Nevertheless, it was imperative for the child to obtain work immediately; and it seemed hardly likely that the child would at once find an employment which would fall in with all these negative requirements. We feel, too, that it should be borne in mind that many of these conditions may be no more than temporary. The child's health or strength may improve; and it seems unjust that, in virtue of his condition at one particular time of his life, he should be for ever dissuaded from taking up an occupation for which he might otherwise seem fit. Indeed, in numerous cases we believe it to be desirable, could it only be arranged, that such children should be re-examined after an interval of three or four years, and that the recommendations should then be reviewed and revised.

IX. Methods of giving Advice.

By MAY SMITH and LETTICE RAMSEY.

General Considerations.

When all the tests had been given, the results worked out, and the home visiting concluded, the investigators met to discuss the occupation to be recommended to each child. Notes were made on each of the one hundred children tested, summarizing their performances in the tests already described, and combining the personal impressions of each investigator and the inferences from the home conditions. The problem then was to decide what occupation seemed most nearly suited to the abilities and temperament of each one, so far as it had been possible to ascertain them by tests and personal observation.

In its general lines the procedure adopted was based on that worked out by Professor Burt and his colleagues at the Vocational section of the National Institute.[1] He has described it as a method of 'progressive delimitation.' Starting first of all with the factor that seems the most general and therefore the most widely influential, and then proceeding step by step to so-called group-factors, and ultimately to factors quite specific, whole

[1] For the general principles involved, *see* Burt, 'The Principles of Vocational Guidance,' *Brit. Journ. Psych.*, *XIV* (1924), pp. 336–52.

classes of unsuitable occupations are gradually eliminated and the choice in the end is narrowed down to one or two particular types of employment.

Thus, broadly speaking, the final decision is reached through three or four distinguishable stages:

(i) Every child was classified, first of all, according to the general level of his native intelligence. For this our main guide was the child's mental ratio in the Binet tests, averaged with or adjusted by his mental ratio in the Performance tests, and at times corrected in accordance with the statements of teachers and parents, and the personal impressions gained during our interviews. He could then be placed in one of the eight classes enumerated in Table IV. This at once limited the scope of reasonable choice. The alternatives open to him were reduced by four-fifths. Instead of the one hundred or more different types of work enumerated in the table, we now had to choose at the outside between twenty or twenty-five.

(ii) The child was next considered from three points of view: (A) his verbal or linguistic ability as shown by his achievements in the Binet tests and in the tests of scholastic attainment; (B) his practical or non-verbal ability, as shown by his achievements in the performance tests, supplemented by reports on his handwork and on his common sense in the practical affairs of every day life; (C) his social ability—ability to get on with others—as reported by teachers and parents and assessed in the personal interview. From each of these three points of view the children were then sub-classified, and each was tentatively allocated to a corresponding occupational group.

(iii) At this stage certain more specific factors were brought forward, as likely to limit still further the final recommendation. Of these the more important may again be sub-divided into four groups:—

1. *Special Abilities:* for example, those needed in scholastic or in manual and mechanical work, or in dressmaking; or, again, such specific capacities as may be loosely summed up in the terms memory or imagination.
2. *Special Temperamental Qualities:* using the term temperamental in a broad generic sense (for example, happy or unhappy disposition, choleric or phlegmatic moods) and including such personal qualities as originality or initiative, and such miscellaneous points as the child's special interests.
3. *Physical Conditions*, whether of a positive or negative implication.
4. *Home Conditions*, with special reference to the immediate need for remunerative employment, the parents' wishes for the child's future, and the openings accessible in the neighbourhood.

(iv) In a majority of cases, it was found that these successive considerations converged ultimately upon a single kind of employment as being the only one consistent with the child's individual requirements, with perhaps two or three alternatives in an order of diminishing suitability. The problem in its final stage resembled one of vocational selection rather than of vocational guidance. Assuming, as it were, that the child has become an applicant for the employment thus indicated, the psychologist proceeds to analyse the candidate's suitability from this new standpoint. He can now, if necessary, apply special selection-tests for that occupation, or re-investigate in greater detail the child's power to meet all the needs of the occupation proposed Thus, without applying all the selection-tests to all the individuals, the fitness of each one for whatever has been suggested can be checked and verified, and the final recommendation can be confirmed.

These, at any rate in theory, seem to be principles upon which vocational advice can best be given. Already in the table above[1] we have enumerated the commoner occupations suited to each distinguishable grade of general intelligence. We need only add that our uniform practice has been to turn first to the highest form of employment at which each child might reasonably aim.

The children falling into the three subordinate groups, marked by high development of some one special quality—linguistic, practical, or social—deserve illustration here at somewhat greater length. The following are typical cases :—

(*a*) Those children who fell in the top half of group A and who were not particularly high in group B or C (*i.e.*, those who were distinguished only by a marked degree of linguistic intelligence) were, as a rule, recommended to take up *clerical work*, always provided that they had reached a sufficiently high scholastic standard and were not unsuited by temperament to a sedentary life.

A.B., for example, had a mental ratio in the Binet tests of 121, but only reached 87 in the non-linguistic tests. Of the 24 girls who were tested in her school, she ranked 4th in speed of arithmetic, 8th in oral arithmetic, 5th in speed of writing, and 8th in spelling. Her social qualities were not well developed; she found it difficult to make friends, was not interested in playing with other children, but preferred to read at home; she was reserved and quiet. She was advised to take up office work, and to attend classes in book-keeping and accountancy.

(*b*) Those falling in the top half of the second group, B (*i.e.*, those of marked non-linguistic abilities) were advised to take up a skilled trade, the particular trade recommended depending upon special abilities, temperamental factors and home facilities.

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[1] *See* above page 16, Table IV.

For example, *C.D.* had a mental ratio of 112 in the performance tests, and a mental ratio of 85 in the Binet tests. She was good at the dressmaking tests, and was anxious to enter that trade. She was recommended to do so.

E.F. had a mental ratio of 97 in the performance tests, and of 78 in the Binet tests. He was an imaginative boy, but rather shy, and did not get on particularly well with others. His scholastic standard was low. He was, however, interested in work with his hands such as carpentry, and showed some constructive ability. He was advised to take up cabinet-making.

(*c*) Those belonging to the top grade of group C (*i.e.*, those who showed a marked degree of social ability and were likely to be particularly successful in dealing with people) were advised, in most cases, to take up some kind of employment where this gift would not be wasted, even if they were, in other respects, fitted for clerical work or the skilled trades. Of the girls of moderate ability, many were advised to be shop assistants or waitresses ; and of the boys several were advised to become shop assistants, waiters, or 'bus conductors.

G.H. had a mental ratio of 117 in the Binet tests and 108 in the performance tests. She had imagination and very good constructive ability. She might have been advised to take up clerical work; but her scholastic standard was not particularly high, and her personal qualities—a good appearance and good manner and a capacity for "getting on" with people—added to the fact that she was not interested in "bookish" things, seemed to indicate that she would be more successful as a shop assistant, particularly if she were placed with a progressive firm giving her a chance to gain a good position.

J.K. had a mental ratio of 96 in the Binet tests and 100 in the performance tests. He was rather small and childish for his age, good looking and with good manners. He was energetic, bright, and inclined to be sprightly and even mischievous. He was advised to start as a page boy in a big hotel.

There were several cases, less well-marked, who fell in the lower half of each group, but who, at the same time, showed more ability in one group than in either of the other two.

Those whose linguistic side was most predominant, but nevertheless not highly developed, were recommended for the lower branches of clerical work, such as that of a stock room clerk or goods checker. Those showing moderate non-linguistic abilities were advised to take up factory work or packing, the latter especially if they showed any ability in judging form relations.

Those who possessed social abilities and were willing and good-natured, but were not remarkable for intelligence as estimated by the tests, were considered suitable for such occupations as house boy, assistant in a small shop, etc.

The following table illustrates the recommendations made in accordance with these principles :—

TABLE XXXIV.—*Occupation Recommended according to Analysis of Ability.*

	A.—Linguistic Ability.	B.—Non-linguistic Ability.	C.—Social Ability.
High Intelligence.	Clerical work of superior type (e g., bank-clerk, if combined with arithmetical ability ; shorthand and typing or secretarial work, if combined with literary and spelling ability).	Skilled trades (engineering, instrument-making, cabinet-making for boys; dressmaking, millinery for girls).	Show-room assistant or shop - assistant in good West-end firms, waiter or waitress in better-class establishments, domestic service in households of the best type.
Moderate Intelligence.	Routine typing; checking in warehouse or stockroom.	Unskilled manual work, (factory work, packing).	Shop assistant (in smaller shops), house boy, page boy, nursemaid, domestic service generally

Special Cases.

When occupations had been suggested for the children who could be placed in the groups just described, there still remained a few who, for one reason or another, could not be placed in any of these groups, children in whose case the accessory factors enumerated above were more important than the primary considerations of linguistic and non-linguistic intelligence and social ability.

Such cases are more frequent than might at first be thought. Of our own cases as many as 24 per cent. required special consideration on these grounds; nor is our experience exceptional. A recent committee of inquiry reports that its members were "seriously concerned to find that, of those on the Exchange registers at the dates of the inquiry, no less than approximately 30 per cent. were classed as presenting special difficulty in placing because they were below normal, physically or mentally, or were of inferior type and neglected appearance: . . . . this," it is added, "is a matter which requires special attention."[1] Even if vocational guidance by means of psychological methods should prove impracticable as a general scheme for all children leaving the public elementary schools, we believe that much might be done by referring such special or exceptional cases to a psychologist or psychological institute for study and advice.

[1] *The London Advisory Council for Juvenile Employment: First Annual Report*, 1925, p. 19.

These special cases (so far as we encountered them) may be divided into four types. The outstanding characteristics of each were as follows :—

(1) Marked special ability, or peculiar talent.
(2) High all round ability, with social handicaps at home.
(3) Low general ability, not amounting to mental deficiency, but often aggravated by poor physique, and uncompensated by any special aptitude or qualification.
(4) Special temperamental difficulties.

1. Children showing special ability were not particularly difficult to advise. But it was often difficult to discover situations that would use the ability in question.

One girl had a mental ratio of 117 in the Binet tests and 95 in the performance tests. She did well in the scholastic tests, and might at first sight have seemed suited to clerical work ; but further testing showed that she was exceptionally good at dress-making and designing, and possessed considerable originality and real keenness for the trade. She was advised to try for a place with a progressive firm of dressmakers where she would get a chance of learning the higher branches of the work and obtaining speedy promotion.

Another girl had a mental ratio of 110 in the Binet tests and 97 in the performance tests. She was not particularly good on the scholastic side; but had an exceptionally good immediate memory. She had a sharp ear, and quick and dexterous finger-movements. Accordingly, she was advised to become a telephone operator.

2. Those who possess a high degree of all round ability often succeed without any outside assistance ; and it might perhaps be thought that they would do well whatever they take up. In actual practice, however, their high intelligence often remains unnoticed ; and they drift into occupations that afford no scope for advancement. In many such cases several alternative suggestions were made : the poverty of the home, however, often rendered it difficult to carry them out.

3. Those who showed no ability in any direction were perhaps the most unsatisfactory group of all. Either the tests had been at fault in not discovering any special tendencies, or the child had such an indefinite character and so little intelligence that no very specific recommendation could be made.

The most that could be done was to name some easy mechanical employment, where the child would not disappoint those who engaged him, and would not suffer the strain or worry of a responsible post to which he was in no way equal. Too often, however, physical infirmity made it difficult to recommend him for the coarse or heavy labour for which otherwise he would seem best suited.

4. Cases of special temperamental difficulty, often showing delinquent or neurotic tendencies, required the most careful consideration. To give a really successful recommendation further investigation would probably have been necessary; but an attempt had to be made with the material at hand. In most cases we had to rest content with some negative conclusion—for example, that the child should not remain at home or should not take up routine work, or should not undertake work where too great demands might be made upon his weak nervous or moral condition.

Several weeks after all the children had been discussed and recommendations decided upon, the whole list was looked through again to make sure that no undue bias had been given to any one factor. Very few changes were suggested. The following table shows the actual recommendations made.

TABLE XXXV.—*Analysis of Occupations Recommended.*[1]

Type of Occupation.	Boys. Occupation.	No	Total.	Girls. Occupation.	No.	Total.
Highly Skilled ..	Business training ..	2		Business training	1	
	Draughtsmen ..	2		Secretarial work	1	
			4	Dress designing	1	3
Skilled . .	Office work (clerical) .	3		Office work (clerical) ..	7	
	Office work (bookkeeping)	3		Shop assistant (superior) .	2	
	Engineering . ..	2		Dressmaking (superior) .	1	
	Motor trades .	2		Millinery .. .	1	
	Cabinet making ..	2		Hairdressing	1	
	Motor body building	1		Manicurist .. .	1	
	Piano making .	1		Telephonist	1	
	Telegraphist	1	15	Photography	1	15
Semi-skilled ..	Shop assistants	3		Needlework trades .	8	
	Motor repairing . ..	3		Shop assistants	4	
	Carpentry or woodwork ..	3		Office girls	2	
	Office boys	2		Nursemaids	2	
	Waiters	2		Domestic service (superior)	1	
	Omnibus company ..	2		Waitress	1	
	Navy or R.A.F. . .	2		Upholstery .. .	1	19
	Zoological Gardens ..	1	18			
Unskilled ..	Factory	3		Factory	5	
	Van boy	3		Domestic service . ..	3	
	Packing	2		Laundry	1	
	Page boy ..	2		Packing .. .	1	
	Call boy	1		Farm or dairywork ..	1	11
	House boy	1				
	Porter	1				
	Army . ..	1				
	Colonies . .	1	15			

Finally, a letter was sent to each of the parents stating the occupation primarily recommended, with, in most cases, one or two alternative suggestions. The chief reasons for each recommendation were given in brief and simple terms; and the letter was intended to be used as a reference if so desired. Below is a specimen copy of a letter. (The portion here printed in roman type was printed on the form, and the part here printed in italics was filled in for each child). Several replies to these letters were received, either asking for employment, or saying that the work recommended had been found and thanking us for the advice.

[1] In this table the side-headings have been entered merely to make our grading of the occupations recommended comparable with the gradings used elsewhere (*e.g.*, on pp. 4 to 7 and on p. 92). The general meaning we have given to each term is sufficiently indicated on p. 16. With children, however, such a classification must not be too strictly pressed. Here, for example, a 'highly skilled' occupation merely indicates an opening that should lead to highly skilled work in the future.

From

The Industrial Fatigue Research Board. | The National Institute of Industrial Psychology.

Dear Mrs. (*Brown*),

I think you already know that a set of special tests have been given to the children leaving *M*——— *St.* school, with the object of finding out the work for which they are best suited.

The results of these tests suggest that your son *James* would be likely to do well at *Electrical Engineering or Motor Engineering.*

He has *good average* intelligence and all round ability: but is, on the whole, best at *mechanical things.* He is *careful and deliberate.*

His writing, spelling and arithmetic are not up to the standard required for office work. He will do much better on the practical side where he has plenty of ability and should make good progress.

I shall be very much interested to hear later on how *James* is getting on.

If this letter is of any use to you as a recommendation, please use it as such.

Yours faithfully,

L. C. RAMSEY.

In 47 cases out of the 100 a different employment was recommended from that originally suggested by the parents or (in a few cases) by the teachers ; in 35 cases the employment recommended was the same ; in 15 cases the parents had had no suggestion to offer ; in 3 cases the parents' wishes were unknown.

It will be seen that, in a large proportion of the cases, the recommendations made by the investigators appeared to coincide with the suggestions of the parent or the teacher. It may be asked, therefore, what special advantage is reaped by a prolonged psychological study of each individual child.

A review of our detailed results shows that, even where our views were in agreement with those of parent or teacher, the parent and often the teacher in the opinions they expressed were apt to be more general and vague ; the tests and the intensive examination, on the other hand, lead to suggestions that are quite specific. A teacher, for example, will recommend the child for 'manual work' or for 'clerical work,' without further particularization ; while our own investigation would commonly show that it was only for certain special types of such work that the child was really suited. The teachers' recommendations seemed implicitly to assume some form of faculty psychology, and to ignore both the very limited character of specific abilities, and the supreme importance and the wide range of general intelligence. Further, we were able to give in order of preference two or three distinct recommendations ; so that, if one type of occupation proved inaccessible owing to the temporary conditions of the trade, a different one might conceivably be taken up.

Where our opinion differed from that of the teacher or parent, it was a great advantage to be able to produce the child's test records and specimens of his actual work. The teacher was usually open to persuasion by a mere statement of results ; but the parent was often more obdurate—sometimes hoping against

all reason that his child would succeed in some high-class, well-paid branch of employment. Nothing but a display of the child's actual performances, side by side with standard results, would convince such a parent that his child was (to quote actual cases) a hopeless misspeller, or had no skill of any commercial value in sewing, drawing, or calculation. Provided our unfavourable conclusion did not seem to him to be mere opinion, and that we had in addition a positive suggestion of our own, the parent always proved grateful for our contribution to the problem. Thus, in nearly every case of divergence, we were ultimately able to carry conviction with both parents and teachers, who might otherwise, for quite irrelevant reasons, have urged the child to go into some employment, which seemed superficially attractive but for which the child was really unfit.

In all these discussions and recommendations we found ourselves labouring under one great disadvantage. Hardly any accurate information exists as to the real needs of the occupations open to young people. We could produce a detailed psychological description of each child; but we had no such psychological description of his prospective employment to compare with it. Some of us, indeed, had enjoyed a first-hand experience of the conditions in large firms and business-houses employing boys and girls fresh from the schoolroom; but it is obvious that no one group of advisers could possibly have a close acquaintance with trades and businesses of every type. What personal knowledge we ourselves possessed could, indeed, always be supplemented by the information readily obtainable from officials of the care committee or of the employment exchange; and for occupations of a less familiar type we could refer to the numerous handbooks and reports issued by the Board of Trade, by the Ministry of Labour, or by private publishers. Nevertheless, time after time, when we were discussing the placement of a particular child, we found that the point most commonly in dispute was not so much the needs of the child himself as the needs of the employment to which it was proposed to send him. What is known as job-analysis, therefore, is one of the most urgent lines of research to be undertaken before vocational guidance can be placed upon a sound footing.

Already we have noticed what great help is afforded at the outset by a rough grouping of the commoner occupations according to the level of intelligence required—by some classification of trades or trade-processes like that given above in Table IV. Further, for particular callings, such as those of the dressmaker or clerk, it proved invaluable to have before us, precisely and in terms of our own standardized tests, what would be the probable wants of a good West-end firm or of an ordinary office in the City. A complete specification along these lines is needed for the several forms of employment most frequently taken up by boys and girls. A general description of each trade, such as those given in the popular handbooks, is not sufficient. Within a given trade

there are countless different operations that call for very different psychological capacities. A teacher, for example, would often tell us that a particular girl " would make an excellent saleswoman " ; but advice of this sort is far too vague for a satisfactory placing: within one and the same general stores the assistants who sell remnants across the bargain-counter need a temperament and personality almost the opposite of the assistants who sell . ats in the showroom to customers more leisurely and wealthy. As a starting point, a classified list of openings, with a simple objective description of the processes to be carried out by the employee, and a note of wages, prospects, length of training, and general conditions of work, would be extremely helpful. But what is wanted most of all is an analysis of the necessary qualifications in exact psychological language, in terms of mental aptitudes that can be precisely defined and, if possible, exactly measured.

The general psychographic scheme which we found workable in our experiments on vocational guidance would doubtless form a useful framework for the analysis of particular jobs: it provides at any rate, a summary or outline of the more outstanding qualities whose variations make up a given personality, and seems at once systematic, comprehensive, elastic, and precisely definable. Time alone would, of course, prevent testing every child for each conceivable quality. But it would doubtless be possible to discover a few key-qualities (as they might be termed)—comprehensive capacities like intelligence—whose determination would at once limit the range of occupational choice. Tests for these could be given to all as a preliminary: after which the procedure would resemble that for selection rather than for guidance.; and a special examination for the more limited capacities needed in particular trades could be carried out at a supplementary sitting, once a broad and tentative classification of the children had first been made. The initial classification might thus be based on group-tests which every child could take (supplemented, of course, by reports from doctor, teacher, and home-visitor); the second examination would then consist in an individual interview, into which special tests would be introduced differing for different children.

It would also, we believe, be particularly helpful if the vocational adviser could know what are the commoner sources of dissatisfaction experienced by employers—what, for instance, are the trades or trade-branches which show the largest labour turnover among juvenile workers, and how far the changes of employment are due to unfitness. According to the *Annual Report of the London Advisory Council for Juvenile Employment*,[1] between 30 and 40 per cent. of the children leaving the jobs found for them, do so on account of inefficiency, bad conduct, or their own dissatisfaction. Whether the child in casual conversation at the exchange is likely to give the real

[1] *Report* for 1925, p. 19.

reason may perhaps be doubted; but an extensive study might be successfully undertaken in a group of cases where vocational maladjustment is indicated or suspected.

These, then, were the principles on which our recommendations were based, and these were the chief difficulties which we encountered.

X. Results of Recommendations.

By Lettice Ramsey.

In the long run, the value of any scheme of vocational guidance can be determined only by following up the after-careers of the children examined. Is there any evidence that the boys and girls who were tested psychologically, and whose choice of employment was guided by the tests, are themselves better satisfied with the occupations they have entered than those who found employment in the ordinary way? Do they, after actual trial, find the work congenial—better suited to their aptitudes and more accordant with their interests? Are they more efficient at it? Do they give greater satisfaction to their employers? Have they in front of them a better prospect than they would have had without the special aid and advice? These, after all, are the crucial issues.

It will be readily understood that the number in our group was too small to yield conclusive answers to such questions. Indeed, it was our modest aim to demonstrate first of all the feasibility rather than the ultimate value of our methods.[1] However, it may prove instructive to see what results have been achieved, even upon a scale so slender.

With this object in view, the homes were all revisited in November, 1925—two years after the date of our original recommendations. The ages of the children now lay between sixteen and sixteen and a half. Sufficient time had passed for almost all to find situations, and to learn whether they liked such work as they had secured.

So far as possible, the following points were ascertained for each child: (i) present employment; (ii) previous employment or employments, if any; (iii) wages; (iv) prospects; (v) satisfaction or dissatisfaction with present and past employments; (vi) reasons for changes.

In most cases the information was supplied by the parents. In half a dozen instances, where the parents themselves were absent or inaccessible, the facts had to be collected from older brothers and sisters, or from remoter relatives and friends. No

[1] For this reason, as more detailed tests and inquiries suggested themselves, we allowed our initial investigation to continue after the date of the school conference; and transmitted our final recommendation, not to the conference, but to the parent. This doubtless has affected the results to be discussed in this section. In any future work it would be desirable, if it were only practicable, to commence the testing at a much earlier stage, so that (as we ourselves had originally arranged) the school conference might also have the benefit of suggestions resulting.

systematic attempt was made to seek information from the employers. It appeared extremely difficult to get access to any persons in the firms who might know the individual children long enough or well enough to state whether each was really giving satisfaction. Data were gathered in regard to any increase of wages or promotion, and (where the child had left) as to grounds for dismissal. For the rest, the firms and factories where the children were employed were too widely scattered to make it worth while to pay special visits to each place of business with a hope of eliciting trustworthy statements.

Every effort was made to eliminate the possibility of undue bias. Throughout, it will be seen that the classification adopted was based entirely on the replies of the informants, not on the views of the visitor. At first, it is true, the parents were inclined to give some vague and optimistic answer, such as "he's getting on quite nicely." But this affected each group alike. And, after a little tactful questioning, every one of them seemed ready to speak quite frankly, and to report progress or disappointment with an equal sincerity. Accustomed as they were to inquiries of this sort, none realized at first that the visitor now paying the call was identical with the investigator who had sent the special letter of recommendation; more often than not, they had in the interval wholly forgotten that any specific investigation had been made other than the usual inquiries preceding the ordinary school conference. Hence there seems no likelihood that their answers were influenced by any recollection of the advice they had received.[1]

Employments Obtained.

Out of the one hundred children who formed the material for our investigation, all except six were successfully traced; and all but two of the number were found to be in work; 25 were engaged in unskilled work, being mainly factory workers or van boys (in the case of the youths), and domestic servants or factory workers (in the case of the girls); 35 were in semi-skilled work, for the most part as needleworkers or assistants in local shops; 26 were engaged in skilled work such as millinery, dressmaking, piano-making, making parts of motor bodies or motor-cycles, or clerical work of various kinds; 6 fell into the category designated above[2] highly skilled, being either engaged upon clerical work of

[1] Owing to the time that had elapsed, and the number of cases passing through her hands, the investigator herself retained little recollection of the occupations originally advised for each child. In order to exclude the possibility of unconscious suggestion, it seemed advisable to abstain from noting whether a child's actual placing corresponded with the original advice, until the information about his efficiency and satisfaction had been duly secured; similarly, the classification of jobs obtained into 'recommended' and 'similar to recommendation' (Table XXXVI.) was made by occupations only, before any note of 'satisfied' or 'dissatisfied' was entered against any particular child's name, and was revised by a second investigator who knew nothing of the classification according to 'satisfaction.'

[2] For the definition of this and the preceding categories, and for further details as to the employments classified under each, see pages 8 *et seq.*, and Tables I to IV.

a superior kind, or else being apprenticed or trained for work of a highly skilled nature.

On the whole, the general nature and distribution of the occupations actually obtained is not very dissimilar from those of the occupations advised.[1] It is true that among the former the amount of skilled employment is somewhat less than among the latter. But many who started in unskilled work have already risen to skilled work, and more will doubtless do so later on. On the other hand, the prospects for the numerous messenger-boys and errand-boys (one or two of them highly intelligent) do not seem hopeful. Generally speaking, however, the number of blind-alley occupations is small; and, even with children of higher ability or special talent, the problem of adjusting demand and supply has proved far less difficult than we ourselves had anticipated.

As many as 30 per cent. had been successful in finding employment of the type primarily recommended by us on the basis of our tests and other inquiries; 22 per cent. had found work which was either similar to the type recommended as a second alternative, or allied in its general psychological nature to one or other of the occupations that had been advised. Rather less than half—40 per cent. in all—were in occupations neither recommended to them nor even similar to those so recommended; large as it is, this proportion is much smaller than we had expected.

Wages Obtained.

The average weekly wage of those in occupations recommended to them, or similar to those recommended, was 16*s*. 9*d*., that of those in occupations dissimilar to what had been recommended was 15*s*. 8*d*. Thus, the former were on the average earning 1*s*. 1*d*. per week more than the latter.

At first sight it might be thought that those who had been unsuccessful in finding work of the kind advised had been forced to content themselves with work of an inferior order. This was sometimes true, but was not the real explanation of their lower wage. Those who had taken up inferior work had been in a few instances tempted by better pay; one or two of the messenger boys, for example, were receiving so much as 24*s*. a week—five or six shillings more than the average wage of the boys.[2] It seems

[1] The percentages in the several grades correspond tolerably well with those of Table XXXV, and, naturally enough, with those of Tables I and II. But to this correspondence too much importance should not be attached. It is more gratifying to note that the quality of the work obtained (clerical, mechanical, social, etc.) is much the same in the two lists. With groups so small, however, it scarcely seems worth while to print any detailed analysis at this stage.

[2] The average weekly wage of the boys was 17*s*. 9*d*.; that of the girls, 14*s*. 8*d*. The maximum wage was 30*s*., and the minimum wage 9*s*., both being earned by girls, the former in a "recommended" and the latter in a "non-recommended" job. It is suggestive to note that if the children at work are divided into two groups according to intelligence, then the average wage of the more intelligent proves to be 18*s*. 9*d*., and of the less intelligent 13*s*. 1*d*. But the factors determining wages are so numerous that statistics derived from a group so small can have but little significance. We record the facts observed for what they may be worth.

clear from a study of the individual cases that the best wage-earners are usually those who have been most rapidly promoted, and have obtained the quickest and the largest rise in pay, and, so far as our meagre data go, we are left with a strong impression that the child's success in his trade or shop was largely attributable to the fact that it had been selected for him so carefully.

Change of Employment.

Of the whole group only 35 per cent. had remained in one and the same place since leaving school. Of those who had been forced to disregard our advice as many as 61 per cent. had had two or more places; and as many as 12·5 per cent. had had four or even five places. Of those who had found work similar to that advised, 43 per cent. had had two or more places; only a single child had had four places; none had had five.

The difference in the proportions obtaining two jobs or more is, perhaps, too small to possess much significance. But the reason for the change was nearly always instructive. Of those who, at the time of the last home visit, were doing work similar to that recommended, but had nevertheless had two or more places, almost exactly half had changed because they did not like the work which they had first taken up, and had eventually taken the work advised in preference. The other half had changed because they had found more remunerative openings of the same sort.

Of the other group—those doing dissimilar work—six had already been dismissed because they were not efficient at their work[1]; most said they had been "turned off because they were no longer wanted"; and a few had found it necessary to leave because the firm moved too far away from home.

General Satisfaction with Employment.

After all, however, in so short an interval the best criterion will be the satisfaction or dissatisfaction of the child himself or his parents. On this point the replies of our informants proved to be classifiable under three main heads. In fifteen cases it was said that the child disliked his work; in fifty-four cases it was said that the child was satisfied alike with his work, with his pay, and with his prospects; in thirteen cases it was said that the child certainly liked the work, but he or his parents were not contented either with the pay or with the prospects. There were ten cases in which replies were unobtainable or seemed ambiguous or untrustworthy; and eight in which the child was out of work or remained untraced. The distribution of these various cases, grouped according to their success in finding employment of the type advised, is shown in Table XXXVI.

[1] Of those who had found occupations of the kind advised none had been dismissed for inefficiency. The sole dismissal among this series was that of a small and lively boy recommended to become a page; he had been discharged "for sliding down the banisters"—a prank which we do not regard as a symptom of inefficiency, since we were definitely assured by another visitor that the boy not only liked his work, but had also been "quite good at it."

TABLE XXXVI.—*Classification of Children according to Employment obtained.*

	A. In Employment recommended.	B. In Employment similar to that recommended.	C. In Employment dissimilar to that recommended.	Total.	Percentage [A and B together, excluding (4)].	Percentage [C, excluding (4)].
1. Satisfied with work, pay and prospects ..	22	19	13	54	83·6	39·4
2. Satisfied with work, but not with pay or prospects ..	7	0	6	13	14·3	18·2
3. Dissatisfied with work ..	0	1	14	15	2·1	42·4
4 Insufficient Information ..	1	2	7	10	—	—
Total ..	30	22	40	92	100·0	100·0

Of the thirty children in work of the kind we had recommended, it will be seen that not one dislikes the work, and that all find it congenial; seven, however, are discontented either with the wages they are receiving or with the prospects afforded. Of those who found employment not exactly the same, but similar in general nature to the work we had advised, all but one like the work, and are also satisfied both with the prospects and with the pay. The single child who dislikes his work dislikes it simply because he has not got work of the exact nature recommended. His case is rather instructive. At school he was considered a dull boy; but in the tests he showed good practical and manual ability, with a special aptitude for colour work: (mental ratio—Binet tests, 66; performance tests, 99). Accordingly, he was advised to enter some firm of motor-builders as a painter of motor bodies. He himself had formerly thought of going into a piano factory with a friend; his parents, however, found him a job first at a bookstall (which he disliked), and then in the bell-making department of one of the largest West-end firms of furniture manufacturers. "This sort of work," said his sister, who was unaware of the advice we had previously given, "he does not like at all; he can't give his mind to it. He is quite set on painting motor bodies." As his father is now in the motor trade, there seems a likelihood that in the near future he will find what he so much desires.

It is interesting to note that, in fifteen of the cases where our advice was successfully followed, the advice that we actually gave was opposed to the suggestions originally contemplated by

the parents. In each of these cases the parents now state that both they and the children are wholly satisfied that the work recommended was, after all, the work finally chosen. The following is a typical case :—

F.G. was a boy with a mental ratio of 90, as measured by the Binet–Simon Tests, but one of 110 with the non-verbal tests of intelligence. He had no high constructive ability; but in the tests of mechanical ability proved particularly good in judging spatial relations and forms. In temperament he was lively and energetic, of a type likely to take well to outdoor life. He was advised to begin by being a van-boy on the railway, with a view to obtaining a place later on as a packer in a goods' yard. His parents, however, found for him a situation which they at first conceived would be better and more remunerative; and he went for a few months into a firm of electrical manufacturers. He quickly found that the work did not suit him; and, as soon as an opportunity arose, he voluntarily obtained for himself a place as a van-boy on the railway. Here he is now doing very well; states that he likes his work immensely; and hopes to be transferred very shortly to the goods' yard.

Of those who are in occupations wholly dissimilar to what was advised, only thirteen are satisfied with the work, the prospects, and the pay; fourteen dislike the work; six say they like the work well enough, but are dissatisfied either with the pay or with the prospects. Of those who are wholly satisfied, it is noteworthy that the reason chiefly given is that they have exceptionally good employers; and, therefore, are influenced not so much by the nature of the work as by the kindliness of the persons under whom they are working. Two were unable to take up the work advised on account of ill-health, which has overtaken them since the time when the advice was given. Two are highly intelligent girls of placid and unambitious temperaments, content with easy and mechanical jobs. The six remaining were children of average general intelligence with no marked talents, special abilities, or outstanding temperamental qualities.

The following cases are typical of this group :—H.J. is a boy of average intelligence and average linguistic ability, but much above the average in the performance tests (mental ratio with these tests, 125). In addition he showed superior mechanical ability, and had always been interested in handwork. He was advised to apprentice himself at an engineering works. Actually, however, he has obtained a place as an office clerk. His intelligence is good enough for him to give satisfaction; and the pay is excellent. Nevertheless, he states that he would much prefer to take up the work advised, but has hitherto failed to find an opening.

K.L. is a girl distinctly above average in general intelligence with the Binet Tests (mental ratio, 120), but of only average ability with the performance tests (mental ratio, 100). In tests

of manual and mechanical dexterity she is, if anything, below the average. She was advised to take up clerical work; but is unable to afford the training, and has hitherto found no situation where she might learn it. She has only managed to obtain work as a packer in a factory; and states that she is dissatisfied both with the work and with her outlook.

Summary of Results.

The final outcome of our efforts may, therefore, be summarized as follows. Of those who obtained the work advised, all (with one doubtful exception) are satisfied, at any rate with the work itself. Of those who obtained work dissimilar to what was advised, more than one half are dissatisfied either with the work that they are doing or with the prospects open to them. We must, however, again insist that with a group so small nothing but *primâ facie* results can be expected, and that nothing but a rough preliminary review seemed possible or justifiable at this stage.

From a closer study of the work actually taken up by the children, it is clear that the weak part of the scheme lay in an insufficient knowledge of the various kinds of accessible employment which would satisfy the special aptitudes found. This was particularly noticeable in the second group—the group who had not taken up the precise occupation advised but some other occupation similar in its psychological nature. Here it would seem that in many instances the actual opening found by the child, though not wholly dissimilar to that advocated on the basis of our examination, was (as judged by subsequent experience) even more suited to the child's abilities than the particular work which had been primarily recommended. In other words, the results of the tests and of the interviews were often more trustworthy than the advice that was based on them; so far as can be gathered, the former were never seriously mistaken, though the practical suggestions deduced from them might in several cases have been improved, had more information been available in regard to actual openings and their exact psychological requirements.

Whether each child has been guided into the right career cannot, of course, be finally determined by only a couple of years' experience. It is conceivable that new talents or new qualities may emerge later on, and the old one be gradually eclipsed; on the other hand, it is possible that the child's chief characteristics—the general level of his intelligence and the main trend of his more stable interests—were already fixed at the time of our inquiry. Which view is nearer to the truth can only be decided by returning once more, after a longer period has elapsed, and reviewing yet again the progress or failure of each child.

It is hoped, therefore, to maintain contact with as many cases as possible, and analyse the after-histories when the children have settled down into more or less permanent occupations.

XI.—General Conclusions.

By Cyril Burt.

The investigation described in the foregoing pages is to be regarded as no more than a first experiment. With a small group of a hundred children, followed up for a period of but two short years, nothing but tentative results can be expected, and nothing but provisional deductions can be drawn. With this reservation, the main conclusions emerging from the whole research may be summarized as follows :—

1. The general outcome of the inquiry speaks strongly in favour of the methods used. The scheme has proved workable ; the results, unexpectedly successful. Judged by the after-histories of the several children, those who entered occupations of the kind recommended have proved both efficient and contented in their work. As compared with their fellows, they are, on an average, in receipt of higher pay ; they have generally obtained promotion earlier ; they have experienced fewer changes of situation ; and have incurred hardly a single dismissal between them. Over 80 per cent. declare themselves satisfied alike with the work they have taken and with their prospects and their pay. On the other hand, of those who obtained employment different from the kind advised, less than 40 per cent. are satisfied Among the latter group nearly half dislike their work ; and among the former only one dislikes it, and that simply because it is not quite identical with what was originally advised. As has been pointed out above, no great weight can be attached to these figures ; yet, so far as they go, they are certainly encouraging.

2. Perhaps the best established conclusion in the whole inquiry is the number and complexity of the factors involved in any attempt at vocational guidance. Not one, but a dozen or more considerations must be duly studied and weighed, before any accurate decision can be reached upon what particular career a child can best be advised to take up. It is clear that, to be genuinely worth while, such a decision must not be based upon a short ten minutes' consultation ; it must be founded upon an intensive inquiry, wherever such inquiry is possible, into all the conditions of the case.

3. What may be the relative significance of each of the different factors concerned, the present investigation has already in part revealed. Native intelligence, at any rate with children of this age, appears to be of supreme importance in the choice of a career. Shrewd as teachers and other officials are in their judgments of individual children, they still sometimes fail to recognize the wide range of individual variation—the insuperable limitations imposed by inborn dullness, and the wide possibilities that are open to inborn talent and ability. Fortunately, native intelligence is the easiest quality to test.

4. Nevertheless, intelligence is but one of innumerable factors ; and nothing but a continued study of a larger number of young people—a study carried out on an even more comprehensive scale and followed up for a number of years—can assign a proper

weight to each of the particular conditions noted and to each of the methods pursued. Thus, by far the most important recommendation that we have to make as a result of our provisional survey is the imperative need for further research. In particular, we would suggest (*a*) extending the inquiry to rural areas as well as urban, and into central and secondary schools as well as elementary; and (*b*) keeping, for a clearer demonstration of the value of a more scientific approach, a record of results at present obtained by existing methods among carefully selected control-groups.[1] A helpful addition would be to distinguish in each case between what might be called the 'ultimate' and the 'immediate' recommendations, *i.e.*, the kind of opening the child is advised to seek to begin with, and the kind of work the child is advised to aim at later on: obviously the latter recommendation would be no more than tentative, and might be subject to revision from time to time.

5. That investigations of this sort are feasible has been amply proved by the present inquiry; and what general plan and what particular lines such a research may most profitably follow, have been clearly demonstrated by our experiments. The schedule, drawn up for the present investigation, has shown itself to be quite serviceable in practice, and to cover, in its main outlines, the major portion of the field. There are, however, several minor modifications and extensions which the progress of our preliminary experiments has shown to be needful.

6. Any further research on vocational guidance must be based upon thorough-going investigations by means of so-called job-analysis. To study from a psychological standpoint the differences between individual children will be worthless without making a similar study of the peculiar requirements of diffe ent trades and occupations. Such investigations could be usefully supplemented by an inquiry into the actual causes of vocational maladjustment. Approaching the child not before but after he has been placed, the psychologist might well attempt to ascertain, both by examining the child himself and by tactful inquiries at his home and place of employment, what are the commoner conditions which make the child discontented with his job and the employer dissatisfied with the child.

7. Among the more specific conclusions that emerge are those relating to the value and the limitations of psychological tests for vocational guidance.

(*a*) The value of such tests is fully confirmed. Wherever it has been possible to test the tests themselves, the outcome has been entirely favourable. The correlation, for example, between the teachers' estimates of intelligence and the estimates supplied by the tests is never below ·5, and often rises to ·9 or over. More often than not, where we have followed up an

[1] Such researches have since been commenced in London by the National Institute of Industrial Psychology with the assistance of a grant from the Carnegie Trustees, and in Cambridge by the Industrial Fatigue Research Board.

apparent discrepancy between the teachers' estimates and the results of the tests, the latter have proved themselves to be the more trustworthy.[1] A teacher, for example, has marked certain children merely 'average' simply because he had had no opportunity to notice the latent capacity of the child: the child perhaps was quiet and unobtrusive, or showed his intelligence chiefly in some non-scholastic line; and, as a consequence, his high abilities had naturally been overlooked in school. The tests, however, revealed them at once.

For vocational guidance the special value of such tests is twofold. First of all, however great may be the intrinsic value of a teacher's observations, there is no other means of equating the standards of one teacher with another, or of one school with a second: tests thus provide a uniform scale of measurement, capable of almost universal application. Secondly, tests are time-savers: where a teacher's report cannot be obtained, or where the child is a newcomer at the school and has not been under observation long enough, there a brief test lasting for only half an hour is sufficient to give a fairly accurate estimate of his general ability.

The same remarks will apply, not only to tests of intelligence whose value is now well recognized, but also to tests of school attainments and specific aptitudes.

(*b*) At the same time, it is clear that tests can cover no more than a limited part of the field. One point we wish particularly to stress. However perfect, however carefully standardized, tests by themselves mean nothing. To apply a scale of intelligence-tests, and to read off the result in a single formula—a mental age or mental ratio—is but the beginning, never the end, of a vocational examination. There is no foot-rule for vocational guidance that can be put into the hands of teachers or welfare-workers, and used with the ease of a thermometer or a pair of scales. With almost every test and with almost every child, the real value of such methods lies, as our experiments have shown again and again, in the interpretation of the test-results. Wherever it is possible, too, such tests should be corroborated by personal observations, many factors of the utmost importance in choosing a vocation are, in the present state of knowledge, not amenable to direct measurement by any of the tests hitherto devised.

8. Of these additional factors, the most important appear to be qualities of temperament and character. Such qualities are even more essential for industrial life than they are for progress in school work. It is, therefore, eminently desirable that suitable

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[1] This applies chiefly to the tests of intelligence—tests which are admittedly the most efficient, and often seem to distinguish between inborn ability and the results of education where a teacher would naturally note only the joint product of the two. In other directions we have constantly been impressed with the insight shown by many individual teachers into the characters and special aptitudes of the children under their charge.

tests for such qualities should, if possible, be devised. Meanwhile, it is consoling to notice that expert observation of the child's behaviour and performances during the tests themselves has proved capable of supplying to the experienced observer much valuable information upon the child's temperamental traits. It is evident, too, that there is room for large improvements in the technique of the personal interview. Emotional and moral qualities must largely be judged on the basis of personal impressions; and, by applying to the procedure at present in vogue the recognized psychological principles already worked out for all forms of oral interviewing, the methods of obtaining and recording such impressions might be greatly refined.

9. One incidental result, of great importance vocationally, is the distinction between children who have a verbal and a non-verbal bias. Many vocations are but little dependent upon verbal capacities. It becomes obvious, therefore, that a child's ability should not be assessed solely by his progress at school or by his performance in psychological tests of a verbal type. Accordingly, tests for mechanical, practical, and manual abilities are equally in need of standardization.

10. A fact of great encouragement was the extreme willingness shown by teachers, parents, and officials, to co-operate in the investigation. All seemed to realize the importance of careful vocational studies for the children under their care; all gave whatever information they could with the utmost readiness.

As a rule, wherever teachers were able to make vocational recommendations, these recommendations appeared to be remarkably well founded, though at times a little vague and general. In many cases, however, the teachers confessed themselves to be at a loss. It is noteworthy that in regard to occupations requiring scholastic accomplishments (such as clerical posts, for example) the teachers' recommendations were always more reliable than where other callings were in question. In their judgments of manual and mechanical aptitudes, and still more in their judgment of temperamental and moral qualifications, the reliability of the teachers varied enormously from one person to another.

It seems clear that in the near future the teacher will have an important part to play in the work of vocational guidance. The intensive personal inquiries that are involved appear far too prolonged and costly to be delegated wholly to outside specialists. With the deeper interest shown by teachers in the psychological study of the individual child, with the increased instruction in psychological methods given to them during their Training College course, it should soon become possible for a large part of the necessary testing and recording to be undertaken as a piece of the normal work of the school. Nor will this be altogether a new and heavy burden; for, in its general outline, the scheme that has proved successful in vocational studies seems closely similar to that of the case-histories now so often compiled for children who demand special attention and examination on account of dullness, backwardness, delinquency, or nervous disorder.

Somewhat unexpectedly, we found that suggestions put forward by parents were often sound and far-sighted. Not infrequently, however, the parents were without any views or proposals of their own; and readily welcomed any advice. The aspirations expressed by the children themselves at times threw indirect light upon their personal qualifications; but, as serious, practical suggestions, were frequently fantastic. Throughout, it was evident that until actual inquiry was made, very little was known of the real interests of the individual children. This, indeed, is one of the most profitable lines which an intensive investigation can follow.

11. Children of outstanding ability, or of remarkable disability, no matter in what direction, were infrequent in our small group of a hundred. As might have been anticipated from what is known already about the distribution of such qualities, pupils of high intelligence or of special aptitude proved to be exceptional and rare. For this reason, a further study, comprising children in trade schools, central schools, and secondary schools, is doubly desirable. In particular, a careful inquiry seems needed to discover whether all the children capable of profiting by these more special forms of education are detected by the present system of selection, and, conversely, whether that system—depending as it so largely does upon written examinations of a semi-scholastic type—may not miss many instances of high technical, manual, mechanical, or administrative ability, and pick out chiefly those whose talents are preponderantly academic. On the other hand, children whose abilities were below the normal were not so exceptional. Children such as these are likely to present special difficulty in placing; and probably, to begin with, it would be for these cases, most of all, that the practical help of the psychologist would be welcomed by the school conference or juvenile advisory committee.

Of all the branches of applied science, vocational psychology is one of the youngest. Already, however, we could quote, were that our purpose, many an instance, where our tests and our individual investigations had directed a young child, on the eve of leaving school, into an occupation for which he proved admirably fitted, but for which his fitness had not previously been noticed and probably would never have been guessed. But our primary concern has been, not with results, but with methods; and we believe that we have amply demonstrated that such methods are feasible, and that, with the further refinement that renewed research will inevitably bring, they will prove of the utmost value to the individual and to the community, to the employer and to industry as a whole.

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A Contribution to the Study of the Human Factor in the Causation of Accidents.

By E. M. NEWBOLD, B.A.

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PREFACE.

The Industrial Fatigue Research Board during recent years have devoted part of their resources to the study of the personal factor in accident causation, that is to say, the part played by the physiological and psychological qualities of the individual in contradistinction to his physical and mechanical environment.

A summary of most of the work already conducted from this aspect of accident causation is contained in the Preface to an earlier Report of the Board[1], and need not be repeated here. As, however, the present Report, which describes an attempt to apply statistical methods to the study of the subject, is actually an extension of some preliminary work carried out for the Board in 1919[2], the reference to this earlier report may be quoted :

> " In addition to the total number of accidents and the immediate factors in their causation, the question of their distribution and of the causes which under equal conditions of risk render some parts of the working population more prone to accidents than others is of great importance.
>
> An investigation carried out by Greenwood and Woods on data collected in munition factories during the war indicates that distribution of accidents is largely influenced by a special personal susceptibility inherent in the individual and differing from one individual to another. Their report suggests, indeed, that this personal susceptibility may be a much more material factor in accident causation than is generally supposed, for it not only shows by statistical proof that in regard to accidents all workers do not start equal, in that some are more liable to suffer casualties than others, but also affords grounds for thinking that the bulk of accidents may occur amongst a limited number of individuals having a special personal susceptibility to accidents.
>
> The further study of this fundamental question of susceptibility is undoubtedly called for in the interests of accident prevention, since, if the importance of special susceptibility as a factor in accident frequency is confirmed and the qualities which constitute it can be determined, the introduction of some system of selection on the basis of the accident risk of the work becomes a practical possibility."

It will be seen that at the time that the above paragraphs were written, two matters had been selected as the basis for study, first, the collection of further evidence as to the existence of this individual susceptibility and the extent to which it operates,

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[1] Osborne, E. E., Vernon, H. M., and Muscio, B. (1922): Two Contributions to the Study of Accident Causation.—*I.F.R.B. Report No. 19*

[2] Greenwood, M., and Woods, H. M. (1919): The Incidence of Industrial Accidents upon Individuals, with Special Reference to Multiple Accidents.—*I.F.R.B. Report No. 4.*

and, secondly, discovery of the factors underlying it. The Board accordingly decided to initiate further investigation on three lines, namely a second statistical inquiry based on accident records specially kept for the purpose, the intensive study of individuals who have had and who have not had accidents, and laboratory research on the factors influencing accuracy of movement.[1]

The first of these investigations, the direction of which has been assumed by the Statistical Committee of the Medical Research Council, has now been completed by Miss E. M. Newbold with the co-operation of 13 large firms in the country, and the results are embodied in the present report.

On the existence of special individual susceptibility the report is confirmatory of the earlier work carried out by similar methods. The observed distribution of accidents among the populations studied is in every case very different from what would be expected either on the theory of chance incidence, or on the theory that liability is affected by the fact that a previous accident has been incurred. In the words of the Report (page 23):

> "So far as the variation in the numbers of accidents per person goes, our facts are in accordance with the theory that the chance of an accident differs for each person; but they are *not* in accordance with the theory that the chance is the same for each person, even allowing the chance, while remaining alike for all, to vary from time to time during the period of observation."

It might perhaps be thought that the existence of differences in individual susceptibility is so obvious that it is superfluous to prove it by statistical reasoning of some complexity. It is, however, by no means uncommon to meet with an expression of views implying that an accident is an occurrence of strictly extrinsic origin and that the qualities of the victim himself are in no way responsible for its incidence.

But apart altogether from this, the methods adopted in the present investigation have enabled the subject of these individual differences to be treated on a roughly quantitative basis, and information to be gained as to their extent and distribution as well as their mere existence. The main result of this has been to confirm the suggestion that (to quote from the summary of the Report) "the average number of accidents in any homogeneous group is much influenced by a comparatively small number of workers," and this carries with it the important practical conclusion that the elimination of comparatively few specially susceptible workers from "risky" occupations would go far to reduce the number of industrial accidents.

---

[1] A fuller account of these investigations will be found in the Board's Fifth Annual Report. (*H.M. Stationery Office*)

To enable this selection to be made without submitting the worker to the test of actual experience, the personal factors underlying this susceptibility must be explored, and it is with this object that the Board have initiated the second and third lines of investigation, namely the testing and examination of individuals who have and who have not incurred accident, and laboratory research. The present investigation, confined as it is to the examination and statistical analysis of mass data, is obviously less well adapted for this purpose. Even here, however, some suggestive points emerge. For instance, the association of youth with accidents, which is indicated in earlier investigations, is confirmed, and, what is probably a newer point, minor accidents show a positive correlation with minor sickness, a fact suggesting that there is a common factor underlying both.

Much work still remains to be done, in order that further knowledge of these and similar relations may be gained, but the Board think that the limits of knowledge that can be acquired solely by statistical methods have now been reached, and feel, indeed, that one of the main uses of the investigation has been to indicate how far statistical inquiry can go, and thereby to clear the way for the application of psychological and physiological methods to the study of the individual.

As a final practical point, the Board would invite attention to the section on p. 26 (containing Table VII) which describes a statistical test suitable for application to departmental accident returns, in order to show whether the average number of accidents is due to conditions affecting all the workers in about the same degree, or whether it is largely due to a small group of persons having many accidents. Clearly, different remedies for these two cases are indicated In the former case, the accidents probably arise mainly from causes affecting all the workers alike, and the remedy lies in some general alteration of the conditions under which work is carried on ; in the latter, further detailed observation of the small groups affected is called for, and if no special cause is found in their individual conditions of work and the reporting of accidents is satisfactory, then the accidents are largely personal in origin and can be reduced by transferring certain individuals to work involving less exposure to risk.

The Board desire to record their indebtness to the Statistical Committee of the Medical Research Council for the general direction of the investigation and to the managements of the different factories concerned for the valuable facilities given.

15, York Buildings,
Adelphi, London, W.C.2.
January, 1926.

A Contribution to the Study of the Human Factor in the Causation of Accidents.

By E. M. Newbold, B.A.

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LIST OF GRAPHS.

I. INTRODUCTION.

The conditions of our present civilisation tend more and more to make it possible for an accident arising from some trivial cause to involve many in disaster. It is not, however, those accidents which most strike the imagination which are best adapted to the study of the cause and prevention of accidents in so far as this may be related to the human factor. For such a study we need a large number of observations both of accidents and of freedom from accidents, i.e., we want to observe a set of people exposed as far as possible to the same risks and to other conditions, some of whom have accidents and some of whom do not, and see what associations we can trace among them. To get anything approaching to such conditions we have confined our present observations to industrial accidents. But this does not preclude the application of any results or suggestions that may arise to the problem of the determination of the fitness of an individual for any position involving special risk to himself or other people. In dealing with the human factor we do not intend in the least to minimise the importance of the mechanical side of accident cause and prevention, or to show any sympathy with the neglect of the first and most clearly necessary duty of any employer to make all machinery and working conditions as free from risks and as foolproof as possible. Foresight and ingenuity in that direction can go a long way, but very little study of the reports of industrial accidents is enough to show that the best devised safeguards cannot cover the whole field. It is repeatedly pointed out in the Annual Reports of the Chief Inspector of Factories that the great bulk of accidents are not caused by machinery and only a relatively small proportion can be prevented by mechanical safeguards. He estimates that the accidents due to machinery represent rather less than one-third of the total, though there is no doubt that they include a high proportion of the more serious accidents.[1] There is, in fact, a limit to mechanical protection, and there will always "remain a large number of accidents against which no material safeguard can be provided, ... described as accidents belonging to the mental field."[2] In a report[3] on Dock Accidents in the Port of London in 1922, stress is laid again on the need for exercise of greater care and the avoidance of unnecessary risks. "It will be seen that a very large proportion of the accidents originate from causes not concerned with deficiencies in safeguards

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[1] Report of the Chief Inspector of Factories and Workshops for 1922. p. 11.

[2] *loc. cit.* p. 13.

[3] *loc. cit.* (1922), p. 35. *See* also Report of the Conference on the Prevention of Accidents at Docks, 1924, p. 22.

or defects in plants and machinery, and the main hope of reducing the numbers and severity of accidents would seem to be rather in the elimination of hazardous practices than in improvement in standards of safeguarding."

A good deal of work has been done by previous investigators on the study of the personal element in accidents, and much of the more recent has been summarised by Mr. D. R. Wilson in his preface to Report No. 19 of the Industrial Fatigue Research Board. Among the factors discussed in this respect are fatigue, speed of production, psychical influence, individual susceptibility, inexperience, age, alcohol, quickness of sight, lighting and temperature. Some of these may be alike for a whole group of workers, others may differ greatly for different individuals, and it is especially with the latter that we are at present concerned. The differentiation of individuals as regards accident susceptibility was for the first time statistically examined by Greenwood and Woods (1919).[1] Dealing with trivial accidents to women munition workers they found that their data suggested an unequal distribution of individual liability, rather than either the effect of pure chance with equal susceptibility to all, or of original equal liability modified by the occurrence of an accident (see Section 3). The present work arose out of that report and continues more or less on the same lines on wider data from varied industries.

The cause of an accident is hardly ever simple ; it may be mechanical, physical, physiological, psychological, or more probably a combination of some or all of these. The value of mass statistics applied to such complex or, indeed, to any psychological happenings has often been called into question. Even to-day one meets with the same criticisms as were raised more than a hundred years ago when Laplace, Poisson, and Quetelet first began to apply numerical treatment to the psychological and sociological questions comprised under their term " choses morales." These criticisms, so far as they are relevant to our present work, are usually based on the following grounds :—

(1) the complexity of the causes which are due to the human element.

(2) the triviality of the events dealt with—(our accidents here are for the most part slight cuts, bruises, burns, etc).

(3) that accidents—serious or trivial—are just " accidents," and might happen to anyone at any time, and are subject to no law.

The answers to-day to such objections are the same as were given then ; firstly, that there is no essential difference except in increased precision (made possible by suitable notation)

[1] Report of the Industrial Fatigue Research Board, No. 4, " The Incidence of Industrial Accidents upon Individuals, with special reference to Multiple Accidents."

between mathematical and any other logical method of reasoning, or as Laplace put it " La théorie des probabilités n'est, au fond, que le bon sens réduit au calcul."[1] The very complexity of the causes into which the human element enters is a reason *for* rather than *against* analytical treatment. " La société n'est pas comme un instrument de physique, qu'on arrange ou qu'on dérange à son gré, pour l'étudier sous toutes ses faces, dans tous ses rouages et sous le jour le plus favorable. . . . On ne peut donc pas, comme dans la plupart des sciences d'observation, rendre égales à volonté toutes les causes influentes moins une, pour étudier les effets et le mode d'action de cette dernière. Souvent il faut procéder par d'autres voies ; il faut substituer l'analyse à la synthèse, et commencer par prendre le phénomène dans son état le plus général."[2]

The charge of triviality of the data would have some foundation if these minor accidents were themselves alone the subject of our enquiry. We look on them rather as some measure—inadequate though we know it to be—of a rather vague quality which we will examine more closely as we go on, and which we may call " tendency to accident." Such a tendency leads to certain events : in 99 cases out of 100, say, the consequences of these events may be of little or no importance, in the hundredth they may be disastrous, hence the seriousness or triviality of the consequence bears in general no relation to the exciting causes ; we may take as an index of the existence of potential causes of dangerous events any results of such causes quite irrespective of their importance in themselves.

We now come to the third objection—that accidents follow no law. The principle of statistical stability has been defined by Poisson[3] in the following terms : " Les choses de toutes natures sont soumises à une loi universelle qu'on peut appeler la loi des grands nombres. Elle consiste en ce que si l'on observe des nombres très considérables d'événements d'une même nature, dépendants de causes constantes et de causes qui varient irrégulièrement, tantôt dans un sens, tantôt dans l'autre, c'est à dire sans que leur variation soit progressive dans aucune sens déterminé, on trouvera, entre ces nombres, des rapports à très peu près constants." " Cette loi des grands nombres s'observe dans les événements que nous attribuons à un aveugle hasard, faute d'en connaître les causes, ou parce qu'elles sont trop compliquées," and he concludes " On ne peut donc pas douter que la loi des grands nombres ne convienne aux choses morales qui dépendent de la volonté de l'homme, de ses interêts, de ses lumières et de ses passions, comme à celles de l'ordre physique."

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1 Laplace. " Essai sur la Théorie des Probabilités."

2 Quetelet. "Lettres."

3 Poisson. " Recherches sur la probabilité des jugements," 1837.

The truth of this principle in any particular application can be established by experience alone. That accidents do exhibit statistical stability has been illustrated by Collis and Greenwood (1921)[1], not only as regards absolute numbers and age distribution of fatal cases, but also in the distribution according to cause and to the part of the body affected in non-fatal accidents, and as described below we find evidence of the same quality also in our present observations of the most trivial accidents.

Nevertheless, in spite of these considerations, there is no doubt that, having regard to both the nature of the tendency we are trying to investigate and the complexity of its causes, the measurable factors with which we are dealing here are pitiably inadequate and meagre. For this very reason, however, even small associations that are found consistently, in spite of all the masking factors, may perhaps indicate a still closer association between the underlying qualities than is shown by the necessarily inadequate numerical expressions that we are using to represent them.

In the passage quoted above, Quetelet says we must *begin* by taking the phenomenon in the most general way. The present paper is only meant as a preliminary clearing of the ground by making as much use as possible of the mass statistics of individual information that are obtainable without special experiment in the normal course of the work in a well-organised factory, and it is ancillary to the more detailed individual experimental work which is being carried out by Mr. Farmer. It is clear *a priori*, and in doing the present work the point has continually forced itself on us that the scope of the mass examination of the sort of material dealt with here is limited, and that a study of the individual factor in accident causation calls rather for more work along the lines of individual study and experimental psychology (which is outside the province of the present writer) than for an extension of the statistical side of the work. This limitation is not meant, however, to apply to the use of accident statistics of this kind by individual factories, where the necessary details relating to the conditions of work and of the workers are either known or can be readily ascertained. Careful study of such records in different departments either along the lines followed here or in other fuller ways which may be possible in individual cases, would probably often define more closely the profitable field of preventive measures. On the whole, the results of the present inquiry show that a high accident average in a department is usually traceable to a relatively small proportion of those employed, and that *part* at any rate, of the cause is personal rather than mechanical. In cases where the distribution is clearly not a chance one, very little observation in the workshop by anyone thoroughly familiar with the work, or perhaps an experimental interchange of occupations

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[1] "The Health of the Industrial Worker." pp. 176 *et seq.*

or machines among workers, would soon decide whether the cause was mainly personal or mechanical, and so point if necessary to the remedy. On the other hand, a high accident average, combined with a chance distribution among the workers, would point to causes inherent in the conditions of the work and affecting all alike.

2. Description of Data.

The original aim of the present investigation was to examine the incidence of accidents in different industries with special regard to the following points, so far as these can be determined from factory records without special experiment :—

(1) The possibility of establishing in different occupations a characteristic ratio between the frequency of accidents of all kinds and notifiable accidents.

(2) The question of the extent of the existence of individual workers having a distinct tendency to incur accidents.

(3) The statistical correlation of accident incidence with age, sex, experience, health, output, etc.

A large number of firms, covering many different industries, were visited and their accident records inspected, and it soon became clear that the ratio of notifiable accidents to accidents of all kinds would not be comparable from firm to firm and from year to year, not only because of the great difference in the risk of trivial accidents in different processes of the same occupation, but also because with improving conditions two factors are acting in opposite directions on the figure for the number of accidents of all kinds :—

(1) Their *actual* number tends to decrease with improved conditions.[1]

(2) Their *reported* number tends to increase owing to greater strictness about reporting trivial injuries, to improved First Aid facilities, and sometimes to the introduction of compensation paid over and above statutory compensation.

Consequently this point was dropped, and attention devoted to the question of the existence of individual tendency and the association of this tendency with other qualities. For this purpose, since large numbers of individual records were needed, it was clearly useless to deal with serious accidents alone ; also, as already mentioned, a trivial accident will serve for our present purposes (which are concerned with the tendency to accident rather than

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[1] For instance, in a department employed on packing soap tablets in wooden boxes each girl used to nail up her own boxes as she packed them ; this resulted in a large number of hammered finger tips, etc. The work was rearranged so that one or two girls did all the nailing for the rest, and these accidents practically disappeared.

the consequences), as well as a more serious one. (For this point, see also Section 10.) In this report, therefore, " accident " denotes any injury, however slight, which is recorded as treated either in the Ambulance Room or from Ambulance Boxes.

Out of many factories visited, twenty-two were chosen as being suitable, and willingly undertook to keep records. In a few cases the past records could be used, but in most cases records have been specially kept for us on individual cards (see specimen in Appendix 5.) The periods covered vary from three months to two years, the total number of workers observed was 8,962—6,938 men and 2,024 women, and the number of accidents 16,188. The management and staffs of the various factories have been very ready to help, and have not only put their records at our disposal but in most cases have had the special cards filled in for us by their own staff, and we should like to express our gratitude to them for this help. We are also glad to acknowledge help in the choice of factories from H.M. Inspectors of Factories. The choice of firms and departments was guided by :—

(*a*) Opportunity for many small accidents.

(*b*) Homogeneity of work done.

(*c*) Strict reporting of all trivial accidents.

The manufactures include: Electrical apparatus, textile machinery, motor cars, optical instruments, general engineering, soap and glue, india-rubber articles, sweets, chocolates, ammunition, brass and copper articles, wooden boxes, cardboard boxes and tin boxes.

The information that it was found possible to get for individual workers was: Age, sex, length of time in the factory, number and type of accidents, visits to the Ambulance Room for minor ailments, time lost for sickness, accident, lateness, and other causes, and occasionally output.

Some of the firms who were willing and in some cases began to keep records had to drop out owing to slackness of work in the departments chosen, or to scarcity of accidents or other reasons such as reorganisation of work; also the records from firm to firm have varied very much in completeness. In very few cases was it possible to get any really comparable information as to output, and in many cases lost time and sickness were not recorded, or were so small as to be negligible, hence the many gaps in our tables.

One of two general points may be noted here. The details of the grouping adopted are given in Table I. Perfect homogeneity of work done in any group is very difficult to find under normal factory conditions; perhaps Group II of Factory D and the small group of women in Factory C come nearer to it than any of the others. Where this was not obtainable we have chosen as far as possible occupations where the chance of accident arises from the material handled or other general conditions

alike for all, rather than from machine accidents. In some cases the complete subdivision of occupations would have resulted in groups too small to deal with separately. Hence, in Factory A (motor car making and assembling) we have combined the occupations into three groups, according to the mean number of accidents per person. Group I contains all occupations where this number was 5 or over; Group II, all occupations where it was under 5 but not lower than 3; and Group III, all under 3. In Factory B (soap making) the classification was on the same principle. Group I (women) consists of the only department with an accident rate less than 1·5 in the six months observed, Group II, all other women's departments—and this group is probably the least satisfactory as regards homogeneity; for men the classification was 1 or over, and less than 1. In Factory G (rubber boot and shoe making) the division was into hand and machine workers. In Factory H (tin-box making) no division was found practicable; the accidents here arose mainly from small cuts by the tin, which all alike had to handle. The opinion of the factory officials was in all cases sought as to the most suitable occupations to take or combine. The groups thus to some extent vary in homogeneity, but several of them are as clean in this respect as it is possible to obtain under ordinary working conditions, and, as appears later, there is no essential difference in the main features either of the distributions of accidents or of the correlations found when one passes from one group to another.

Stress must be laid on the fact that no comparison as regards the average number of accidents should be made from one group to another; the conditions are quite different, and such a comparison would lead to no useful result. Our aim has been rather to take each group separately, and examine the distribution and associations *within* that group, and then see what points are common to many groups. In dealing with variables, in which the social or human elements can enter largely, it is often not possible to get any single homogeneous group big enough for an association which is not quantitatively large, to appear significant with regard to its probable error, but a number of consistent results from different groups, even if the sampling errors are large, inspire some confidence in their stability. This method of treatment has involved a good deal of somewhat tedious numerical computation, for the greater part of which I am indebted to Miss C. A. Thomas, Mr. E. Lewis-Faning and Mr. J. Martin.

3. Distributions of Accidents among the Workers.

(See also Appendix I and II.)

To avoid a wearisome description, the main preliminary facts about the different groups have been set out in tabular form in Table I, not for comparison one with another as regards

TABLE I.

Nature of Groups observed as regards Occupation and Accidents (Males).

Factory.	Group.	Nature of Occupation.	Length of Period Observed.	Mean No. of Accidents per Person in Period.	* Standard Deviation	* Observed Coefficient of Variation.	C. of V. of Poisson with same Mean.	No. of Persons Observed	No. of Accidents.
		Making and assembling Motor Cars (Metal Departments):—							
A ..	I.	All departments with mean number of accidents, 5 or over.	6 months	6·44	6·47	100·6	39·4	204	1,313
A ..	II.	All departments, under 5 but not under 3.	Do.	3·78	4·14	109·6	51·4	352	1,330
A ..	III.	All departments under 3 ..	Do.	2·56	3·25	127·1	62·5	304	777
		Soap Manufacture :—							
B ..	I.	All departments with mean 1 or over.	5 months	1·81	2·26	124·7	74·3	148	268
B ..	II.	All departments under 1 ..	Do.	·57	·90	159·7	133·0	299	169
		Makers of Textile Machinery :—							
E ..		Whole Factory	2 years	1·36	2·68	196·3	85·8	3,601	4,914
E ..	I.	Fitting	Do.	3·64	3·71	102·1	52·4	440	1,601
E ..	II.	Moulding	Do.	·51	·97	188·5	139·7	281	144
E ..	III.	Ring Room	Do.	·18	·77	436·5	237·7	226	40
E ..	IV.	Spindle and Flyer	Do.	·41	·83	205·1	156·9	256	104
E ..	V.	Joiners	Do.	2·73	4·34	159·2	60·4	77	210
E ..	VI.	Sawmill	Do.	2·66	3·54	133·1	61·4	93	247
E ..	VII.	Tinworking	Do.	5·60	7·50	134·0	42·3	57	319
		Commercial Motor-vehicle Makers :—							
F ..	I.	Valve making	3 months	·27	·45	163·3	191·5	22	6
F ..	II.	Capstan department ..	Do.	·43	·67	154·0	152·1	81	35
		Rubber Boot and Shoe Making :—							
G ..	I.	Hand workers	2 years	1·47	1·94	132·4	82·8	47	69
G ..	II (*b*).	Machine workers	Do.	1·61	2·38	147·7	78·8	82	132
		Parts for Cars, Motor Cycles, etc. :—							
I ..	—	Machine shop	1 year	·68	1·31	191·8	120·9	190	130
		Chocolate manufacture :—							
M ..	I.	Moulding chocolate (machines), day.	1 year	3·94	3·84	97·3	50·4	301	1,187
M ..	II.	Ditto, night..	Do.	3·98	3·75	94·0	50·1	376	1,499
M ..	III.	Confectionery sugar boiling, mixing cream paste, etc.	Do.	2·50	2·57	102·6	63·21	181	453
M ..	IV.	Unloaders	Do.	·48	·77	159·5	144·2	106	51
M ..	V.	Warehouse, despatching (packing in cases and loading into trucks).	8½ months	4·07	4·26	104·7	49·60	92	374
M ..	VI.	Fitting (General Engineering and maintenance).	1 year	1·95	2·67	137·2	71·70	218	424
N ..	—	Rolling Mills (brass and copper)	6 months	2·90	4·83	166·6	58·7	284	823
		Scientific Instrument makers :—							
P ..	—	Engine room workers ..	1 year	1·04	1·41	135·9	98·1	50	52

* The standard deviation is a measure of the *absolute* variation of a set of observations about their mean It is the squa
of the squares of the deviations from the mean.

The coefficient of variation is a measure of the *relative* variation of the observations about their mean. It is the standar
as a percentage of the mean.

(8820)

TABLE I.—*contd.* (*Females*).

Factory.	Group.	Nature of Occupation.	Length of Period Observed.	Mean No. of Accidents per Person in Period.	* Standard Deviation.	* Observed Coefficient of Variation.	C. of V. of Poisson with same Mean.	No. of Persons Observed.	No. of Accidents.	No. of Accidents per Person per Year (Approx.).	Date of Period of Observation.
		Soap Manufacture :—									
B ..	I.	Toilet soap dept.	5 months	1·06	1·43	134·6	97·1	145	154	2·55	Jan. 5–June 5, 1923.
B ..	II.	All departments with Mean over 1·5.	Do.	2·12	2·36	111·3	68·7	100	212	5·09	Do.
C ..	—	Power press punch hand ..	5 months	2·43	2·79	114·9	64·1	58	141	5·83	Aug 1–Dec. 31, 1922.
		Sweet Making :—									
D ..	I.	Bottling	2 years	5·43	3·94	72·6	42·9	28	152	2·71	Jan. 1, 1921–Dec 31, 1922.
D ..	II.	Dipping nuts, etc., into caramel.	Do.	1·39	1·84	132·9	84·8	98	136	3·3	Do.
		Rubber Boot and Shoe Making :—									
G ..	I.	Hand workers	2 years	·63	1·28	201·7	125·7	120	76	·32	Feb. 1, 1921–Jan. 31, 1923.
G ..	II.	Machine workers	Do.	·52	1·02	197·0	138·7	50	26	·26	Do
H ..	—	Tin box making	1 year	·79	1·16	146·7	112·4	346	274	·79	Jan. 1–Dec. 31, 1922
		Parts for Cars, Motor Cycles, etc. :—									
I ..	—	Machine shop	1 year	·70	1·10	158·6	119·9	161	112	·70	Do.
K ..	—	Tin box making	6 months	1·34	1·80	134·5	86·4	125	167	2·67	April 1–Sept. 30, 1923.
		Chocolate Manufacturers :—									
M ..	I.	Card box making	8⅓ months	·37	·73	198·1	164·8	380	140	·47	Mar. 19–Dec. 31, 1923.
M ..	II.	Wooden box making ..	Do.	2·30	2·93	127·7	66·0	161	370	2·91	Do.
M ..	III.	Wrapping in tinfoil and paper (machines).	Do.	1·06	1·33	124·9	97·0	142	151	1·35	Do.
M ..	V.	Wrapping in tinfoil (machnies)	Do.	·65	1·21	187·5	124·5	110	71	·82	Do.

* The standard deviation is a measure of the *absolute* variation of a set of observations about their mean. It is the square root of the average of the squares of the deviations from the mean.

The coefficient of variation is a measure of the *relative* variation of the observations about their mean. It is the standard deviation expressed as a percentage of the mean.

The full curves give in each case either a single Poisson[1] fitted to the mean or, where the group contains more than one occupation, a combined Poisson as described above. In every case except the small experience (small in both time and numbers) of Factory F, the correspondence with the observed values (shown as histograms in the figures) is bad, and with the exception of the same factory, the discrepancy is always in the same direction, i.e., there are too many people observed with no accidents, and also too many people with the larger numbers of repeated accidents, in other words, as the coefficients of variation showed us, the individuals are not grouped so closely about their means as they should be on the theory of equal risk for all. Men and women show the same tendency.

The general result suggests that perhaps the mean is not the best criterion to take in estimating the theoretical distribution. The average number of accidents in a department is influenced by a comparatively small number of multiple accident people, whose accidents seem to be due either to a special tendency in themselves or to special circumstances in their work or their way of doing it. Since in most cases the largest group is that of persons with no accidents, another not unreasonable way of looking at the question is to take the proportion of this group to the whole as a criterion, and assume that it is not too much to expect that, since this proportion of people can escape accidents altogether, the accidents which do occur in the same department should be distributed according to the Poisson exponential, which has the same proportion in the zero group. The theoretical distributions obtained on this assumption are shown in Graphs I and II by the broken line curves.[2] Here again Factory F is the only one which gives a really good fit, and, as might be expected from the previous comparison, the number of people with many accidents exceeds the theoretical number to a still greater degree. For instance, in Factory A Group III, by the "pure chance" theory we should not expect anyone to have more than fourteen accidents on the basis of the observed mean, nor more than 8 on the basis of the observed number who escape accident altogether, whereas in actual fact there were 114 with over 8 accidents and 30 with over 15, the largest number being 34.

It is very clear from these graphs that our groups are not homogeneous as regards accident risk. We can, of course, never hope to establish the truth of a hypothesis by the accuracy of fit of an observed series with the theoretical distribution calculated on the basis of the hypothesis. Even with very large numbers, excellent fits can be obtained with series based on quite incompatible hypotheses. Such comparison, nevertheless,

[1] Strictly speaking, it may be considered illogical to represent a discontinuous series of this kind by a continuous curve, as the frequencies are here given by the ordinates, but the curves have been used as being less confusing to the eye, and the top of each ordinate marked by a dot.

[2] See also Appendix I.

may be extremely useful to show either that a given hypothesis does *not* hold, or that it *might* hold, and, on the assumption that it does, to see what sort of values would be obtained for constants involved.

Two other hypotheses relevant to accident distributions were discussed by Greenwood and Yule in the paper quoted above. The first[1] of these assumed that the liability to accidents is altered by having sustained an accident; the series from which the numbers having 1, 2, . . . accidents can be computed is $\frac{N}{s}\left(\frac{N-s}{N} + \frac{s}{N}\right)^n$ omitting the first term, where s is given by

$$\mu_2 = \frac{n\{N-n + s\,(n-1)\}}{N^2}$$

N=Number of people.
n=Number of accidents.

μ_2 = the second moment of the accident distribution.

When s is greater than unity it denotes an *increased* liability after the first accident. The group having no accidents is equal to $\frac{N}{s}\left(\frac{N-s}{N}\right)^n + \frac{N\,(s-1)}{s}$

The writers concluded that the theoretical basis of this biassed scheme was not appropriate to the present problem, and that at best it could only give a good smoothing formula. In the simplified form in which we have quoted it, it can be readily employed, and they found that it graduated some of their distributions very reasonably, though not, on the whole, so well as a third scheme. It has not proved so successful with those of our present samples. We have tried it on factories A (all), B (all males), E (all), M Group I males and M Group II males, and in each case it fits badly and gives a second maximum after the first term which does not appear in the observed values. From the form of the equation we see that this later maximum will appear whenever $n-1$ is greater than the integral part of $\frac{(n+1)\,(N-s)}{N}$. This leads to the consideration that if in distributions where, as in most of the present cases, the accident risk is small, so that the zero group is a large one, there were any general tendency for one accident to lead to another for psychological or other reasons, this would probably after a time lead in many cases to distributions with at least two modes, one in the zero group and another later on. Practically none of the observed distributions are of this form; almost all, with the exception of Factory A, Group II[2], have only one well-defined

[1] *loc. cit.* p. 259.

[2] In the single exception the second maximum, which occurs at three accidents, disappears when the distribution is "corrected" for age and sickness tendency (see Section 8). It might be objected that this is what would be expected, and since age and length of time in the factory are positively correlated, it makes the test invalid if a correction is made for age. This would be justified if age were *positively* correlated with the number of accidents, but, as a matter of fact, this is one of the groups which shows an appreciably significant *negative* correlation between age and accidents.

maximum, usually in the group with no accidents, in a few cases in the one accident group or later. This is in itself some evidence against such a tendency.

We must remember also that our periods of observation begin at quite arbitrary and irregular periods as regards the length of previous factory life of the different individuals in each group, and if there were any very distinct tendency for increased liability to accident to those who had already had one or more, we would naturally expect to find some tendency on the whole for those who had been longer in the factory (and so had more opportunity for previous accidents, before our period of observation began) to show increased liability to accident. This is not, however, the case; what correlation there is between accidents and length of previous employment in the factory is, in the great majority of cases, in the other direction, i.e., the people with most accidents have, on the whole, been a shorter time in the factory (see Section 4 below).

The third scheme discussed by the same authors[1] was one arising from the theory of an unequal distribution among the workers of susceptibility to accidents. The simplified form they adopted was based on the assumption that this distribution was continuous and of the form $y=y_0 e^{-c\lambda} \lambda^{r-1}$ where λ is the measure of liability, or susceptibility, and c, r and y_0 constants. The resulting accident distribution is fitted from the mean and 2nd moment about the mean of the distributions observed.[2] They found on their data from women workers in munition factories that this scheme gave a better fit than either the pure chance or the biassed scheme. We have tried this on some of our larger groups with the same result. The details are given in Tables II to V. In those groups, which are made up of more than one department, we have taken as the pure chance theoretical distribution the sums of the Poissons fitted to the means of each department separately. Very few of these equal liability distributions, as we have already seen from graphs I and II, give at all a reasonable fit, and some entirely fail to get the general shape of the observed groups. The unequal liability theory (denoted in the tables by (51) and (52)) is in all cases a great improvement on the Poissons. It is still far from a very good fit in some cases, but by combining some of the groups the results on the whole are fair, as tested by Pearson's Goodness of Fit Test. To sum up these results I think we can say that, though it clearly does not give the whole truth, the assumption of this particular arbitrary smooth

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[1] *loc. cit.* p. 274.

[2] The equations for fitting are, mean $= \frac{r}{c}$, $\mu_2 = \frac{r(c+1)}{c^2}$, and the frequencies of 0, 1, 2 . . . . accidents are given by the successive terms of—

$$N \left(\frac{c}{c+1}\right)^r \left\{1 + \frac{r}{c+1} + \frac{r}{2!}\frac{(r+1)}{(c+1)^2} + \frac{r(r+1)}{3!}\frac{(r+2)}{(c+1)^3} + \dots\dots\right\}$$

TABLE II.—*Comparison of Observed with Theoretical Distributions.*

FACTORY A (Whole Factory). MALES.

No. of Accidents.	No. of Persons. Observed.		Calculated by[1] (51) and (52). (a)		Sum of Poissons[2] to Departments. (b)	
0	230		198·73		38·22	
1	105		137·52		94·04	
2	83		104·38		138·25	
3	90		81·55		149·29	
4	71		64·63		131·58	
5	50		51·65		101·98	
6	47		41·51		73·23	
7	44		33·49		50·24	
8	26		27·10		33·28	
9	16		21·98		21·25	
10	25		17·86		12·97	
11	12		14·54		7·53	
12	13		11·85		4·13	
13	9		9·66		2·14	
14	9		7·89		1·05	28·56
15	3		6·45		·49	
16	6	98	5·27	97·46	·21	
17	3		4·31		·03	
18	2		3·53		·01	
19	1		2·89			
20	2		2·37			
21	1		1·94			
22	2		1·59			
Over 22	10		7·31			
	860		860·00		859·92	

(a) For 11 groups as by right hand brackets. $\chi^2 = 24 \cdot 24$, $P = \cdot 007$
For 8 groups as by left hand brackets. $\chi^2 = 7 \cdot 42$, $P = \cdot 388$

(b) $1245 \cdot 5$, $< \cdot 000001$

[1] On the theory of differing individual liability.

FACTORY A. (Group I.) MALES.

No of Accidents.	No. of Persons. Observed.			Calculated by (51) and (52). (a)		Sum of Poissons to Departments. (b)	
0	41	58		22·86	45·47	0·42	2·95
1	17			22·61		2·53	
2	10	21		20·75	39·30	7·61	23·01
3	11			18·55		15·41	
4	16	23		16·36	30·67	23·68	53·16
5	7			14·31		29·48	
6	18	36		12·45	23·24	30·99	59·31
7	18			10·79		28·32	
8	10	15		9·33	17·37	22·96	39·73
9	5			8·04		16·77	
10	11	18		6·92	12·87	11·17	18·02
11	7			5·95		6·85	
12	5	8		5·10	9·48	3·89	5·96
13	3			4·38		2·06	
14	6	6		3·75	6·96	1·02	1·50
15	0			3·21		·48	
16	4	5		2·74	5·08	·21	0·30
17	1			2·34		·09	
18	1	2		2·00	3·71	·03	0·05
19	1			1·71		·02	
20	1	2		1·46	2·71		
21	1		14	1·25			
22	2	2		1·06	1·97		
23	–			·91			
24	3	8		·77	5·17		
Over 24	5			4·40			
	204			204·00		203·99	

(a) For 13 groups $\chi^2 = 26 \cdot 15$, $P = \cdot 010$ (b) For 10 groups $\chi^2 = 5049 \cdot 1$, $P < \cdot 0000000$

[2] On the theory of pure chance.

TABLE III.—*Comparison of Observed with Theoretical Distributions.*

FACTORY B. (Whole Factory).—WOMEN.

No. of Accidents.	No. of Persons. Observed.	Calculated from (51) and (52).	Sum of Poissons to each Department.
0	111	116·03	80·47
1	66	61·87	84·32
2	42	35·72	53·52
3	17	21·14	26·38
4	10	12·67	11·95
5	9	7·65	5·52
6	4	4·64	2·63
7	4	2·83	1·25
8	1	1·72 } 7·28	0·57 } 2·21
9	1 } 8	1·05	0·24
10	1	Over 9 1·68	Over 9 0·15
11	1		
	267	267·00	267·00
	For 8 groups	$\chi^2=3{\cdot}4$ $P = {\cdot}84$	$40{\cdot}0$ $<{\cdot}000001$

FACTORY B. (Whole Factory).—MEN.

No. of Accidents.	No. of Persons. Observed.	Calculated from (51) and (52).	Sum of Poissons to each Department.
0	239	276·4	202·0
1	98	74·8	130·4
2	57	37·2	65·6
3	33	21·3	29·5
4	9	13·0	12·4
5	2	8·2	4·8
6	2	5·3	1·6
7	1	3·5	0·5 } 0·7
8	—	2·3	0·2
9	4	1·6	
10	—	1·1 } 10·8	
11	1 } 7	0·7	
12	—	0·5	
13	—	Over 12 1·1	
14	1		
	447	447·0	447·0
	For 8 groups	$\chi^2=38{\cdot}5$ $P= {\cdot}000015$	$75{\cdot}7$ $<{\cdot}000001$

Table IV.

Comparisons of Observed with Theoretical Distributions.

FACTORY E. (Whole Factory).—MALES.

No. of Accidents.	No. of Persons. Observed. (grouped)	Observed.	Observed. (grouped)	Calculated by (51) and (52). (grouped)	Calculated by (51) and (52).	Sum of Poissons fitted to means of Departments.
0	2667	1888		2664·73	2115·89	1532·08
1		779			548·84	846·59
2	530	343		477·25	293·44	492·80
3		187			183·81	302·68
4	201	108		210·05	123·57	189·05
5		93			86·48	113·36
6	83	51		107·53	62·11	63·28
7		32			45·42	32·74
8	47	20		58·86	33·66	15·84
9		27			25·20	7·25
10	26	15		33·48	19·02	3·16
11		11			14·46	1·32
12	20	13		19·52	11·04	·53
13		7			8·48	·20
14	10	5		11·58	6·53	·10
15		5			5·05	·03
16	17	2		18·00	3·92	·01
17		1	15	Over 16	14·08	
18		—				
19		1				
20		3				
21		1				
22		1				
23		3				
24		—				
25		1				
26		1				
27		—				
28		—				
29		—				
30		—				
31		—				
32		1				
33		1				
—		—				
—		—				
—		—				
—		—				
—		—				
39		1				
		3601			3601·00	3601·02

For 18 groups $\chi^2 = 147{\cdot}2$, $P < {\cdot}000001$

For 9 groups (as by left hand grouping). $\chi^2 = 16{\cdot}2$, $P = {\cdot}04$

TABLE V. *Comparison of Observed with Theoretical Distributions.*

FACTORY M. GROUP I (Day Shift). MALES.

No. of Accidents.	No. of Persons. Observed.		Calculated by (51) and (52). (a)		Single Poisson fitted to Mean.		Single Poisson fitted to zero group.	
0	42		40·65		5·45		42·00	
1	44		40·28		20·79		74·37	
2	30		34·87		39·64		65·89	
3	30		28·72		50·38		38·94	
4	25		23·06		48·03		17·27	
5	11		18·22		36·64		6·13	
6	12		14·25		23·29		1·82	
7	15		11·06		12·69		·46	
8	8	16	8·53	15·08	6·05	8·61	·10	0·12
9	8		6·55		2·56		·02	
10	8	14	5·01	11·75	·98	1·43		
11	1		3·83		·34			
12	5		2·91		·11			
13	1	8	2·21	9·06	·03	0·04		
14	2		1·68		·01			
15	2		Over 14 5·17					
16	—							
17	1							
18	—							
19	—							
20	1							
21	1							
	247		247·00		246·99		247·00	

(*a*) For 11 groups $\chi^2 = 6{\cdot}52$, $P = {\cdot}77$

FACTORY M. GROUP II (Night Shift). MALES.

No. of Accidents.	No. of Persons. Observed		Calculated by (51) and (52). (a)		Single Poisson fitted to Mean.		Single Poisson fitted to zero group.	
0	54		51·36		7·54		54·00	
1	56		58·16		29·47		104·73	
2	57		53·75		57·60		101·62	
3	53		45·95		75·07		65·78	
4	29		37·68		73·37		31·95	
5	28		30·11		57·38		12·42	
6	19		23·65		37·39		4·03	
7	20		18·34		20·89		1·12	
8	9	20	14·09	24·83	10·21	14·65	·27	0·33
9	11		10·74		4·44		·06	
10	11	28	9·13	18·86	1·74	2·56	·01	
11	7		6·13		·62			
12	10		4·60		·20			
13	3	12	3·44	13·31	·06	0·08		
14	3		2·57		·02			
15	1		1·91					
16	2		1·42					
17	2		Over 16 3·97					
18	—							
19	—							
20	1							
	376		376·00		376·00		375·98	

(*a*) For 11 groups $\chi^2 = 10{\cdot}20$, $P = {\cdot}42$

distribution of individual susceptibility among a body of workers, ignoring the different risks in different departments, gets considerably nearer the truth than does ignoring individual susceptibilities and taking account only of different risks in different departments. This is a noteworthy point, as in some cases the differences in the mean number of accidents in different departments is very sharp. The association between the department in which the worker was employed and the number of accidents he (or she) had as measured by the correlation ratio has been found in a few cases, and is of about the order ·4.

Correlation Ratio between Department and No. of Accidents.		No. of Depts.	No. of People.
Factory.	η		
A. Males	·306 ± ·021	18	860
B. ,, ...	·363 ± ·028	26	447
B. Females	·397 ± ·035	14	267
E. Males	·463 ± ·009	58	3,601

Here again, of course, we must remember that so far as the distributions go, " individual susceptibility " may mean susceptibility due to some conditions in the individual's work which he does not share with the others just as well as personal tendency. In the following sections we shall go into this point, and there are many suggestions that the part played by the personal factor is not unimportant.

Most of our groups are too small to fit curves to with any profit, hence we will go back to another way of expressing the variation which can be applied to them all, and consider among the innumerable possible schemes of sampling the three well-known elementary ones which have been called those of Bernoulli, Poisson and Lexis. To describe these clearly we take up the old parable of the urns :—

(1). *Bernoulli's Scheme.*—We make s drawings from an urn having a proportion, p_0, of white and black balls, the proportion being kept constant by putting back the balls as drawn, and we note the number of white balls ; we do this N times, so that we have N sets of s balls in each set, and know the numbers m_1, m_2, m_N of white balls in each set. Then, if the proportion of white and black balls has been unaltered throughout and the drawing random, the theoretical standard deviation σ_B of the number of white balls drawn will be $\sqrt{sp_0 q_0}$, where $q_0 = 1 - p_0$.

(2). *Poisson's Scheme*[1].—We have s urns containing black and white balls in proportions $p_1, p_2 \ldots . p_s$, constant for each urn, but varying from one urn to another. We make s drawings, one

[1] This is not, of course, to be confused with Poisson's exponential series referred to above, which results here as a particular case of the Bernoulli scheme when p_0 is very small and s very large.

from each urn, again with replacement, and note the number of white balls. We do this N times so that again we have N sets of s balls in each set, and know the numbers $m_1, m_2 \ldots m_N$ of white balls in each set. Then, if the proportions $p_1, p_2 \ldots p_S$ have not changed during the N sets of drawings, the theoretical standard deviation σ_p of the numbers of white balls will now be less than in the previous case. We have, putting

$$p_0 = \frac{1}{s}(p_1+p_2+\ldots+p_S) \text{ and } q_0 = 1-p_0$$

$$\sigma_p^2 = sp_0\,q_0 - \sum_{r=1}^{r=s}(p_r-p_0)^2 \qquad (1)$$

(3). *Lexis' Scheme.*—We go back to a single urn having black and white balls in a certain proportion p_1, and make a set of s drawings from it, replacing as before each ball as drawn, and note the number m_1 of white balls, we then alter the proportion of black and white balls to p_2 and make another set of s drawings, we do this N times, altering p each time, so that again we have N sets of s balls and know the numbers $m_1, m_2 \ldots m_N$ in each set. In this case the constitution of the urn has remained the same for a single set, but has changed from set to set. Then putting $p_0 = (p_1 + p_2 + \ldots + p_N)/N$ and $q_0 = 1 - p_0$, so that p_0 is the average chance of a white ball, the theoretical standard deviation σ_L of the numbers of white balls in a set will be greater than in the Bernoullian and we will have :—

$$\sigma_L^2 = sp_0\,q_0 + \frac{s^2-s}{N}\sum_{r=1}^{r=N}(p_r-p_0)^2 \qquad (2)$$

$$\text{i.e., } \sigma_L^2 = \sigma_B^2+(s^2-s)\,\sigma_p \qquad (3)$$

where σ_p is the standard deviation of the variations in the chances from the mean chance p_0.

Now the N sets of drawings in each case are the N people in an observed group, each single one of the s drawings of a single set is the result of observing a person for one small time-period—so small that only one accident could possibly happen in it. Each white ball drawn represents an accident, a black ball drawn represents the passing of one of the small time-periods without an accident to the person observed. The p's represent the chances of an accident happening to a person in one of the small time-periods. Accordingly scheme (1) corresponds to the theory that the chance of an accident is the same for each person and unchanged throughout, scheme (2) to the theory that the chance of an accident is the same for each person, but changes for all alike at different times during the period of observation, and scheme (3) to the theory that the risk differs from one person to another, but does not change during the course of the period of observation.

Now we know N = the number of people, and we know the numbers $m_1\ m_2 \ldots m_N$ of accidents each of these people has, and so can find the mean (sp_0) and standard deviation σ of these numbers (see Table I). We *don't* know s, and we *don't* know the p's, but we may reasonably suppose the time-period in which only one accident can happen to be very small compared to the whole period of observation, so that s, the number of these periods, is very large. Even if we take into account the time taken to go to the Ambulance Room and have a dressing put on, and allow, say, 5 or even 10 minutes for each accident, the number of 10-minute periods in the whole working time observed is large enough for us not unreasonably to neglect s compared to s^2, and we may also suppose that p is small enough to neglect sp^2 compared to sp. The theoretical standard deviation $\sqrt{sp_0q_0}$ of the Bernoullian scheme thus becomes equal to $\sqrt{sp_0}$, i.e., to $\sqrt{M}$, where M is the mean number of accidents per person. In our groups in every case but one (and that, as we have seen, but a little one) the observed standard deviation is greater than the square root of the mean, so that of the three schemes that of Lexis is the only one tenable. This is only another way of putting the fact already noted that the observed coefficient of variation is greater than the theoretical, and is a not unexpected result.

Hence, so far as the variation in the numbers of accidents per person goes, our facts are in accordance with the theory that the chance of an accident differs for each person (Lexis' scheme); and they are *not* in accordance with the theory that the chance is the same for each person (Bernouille's scheme), even allowing the chance while remaining alike for all, to vary from time to time during the period of observation (Poisson's scheme).

The advantage of thinking of these three schemes is that it leads us at once to the most natural measure of the variation of the chance of an accident from one person to another—the standard deviation of these personal chances expressed as a percentage of the mean. Charlier has called this the " coefficient of disturbancy "[1] and gets at once from equation (2) above by neglecting s compared to s^2

$$\rho = \frac{\sigma_p}{p_0} = \frac{\sqrt{\underset{r=1}{\overset{r=N}{S}} (p_r - p_0)^2/N}}{p_0} = \frac{\sqrt{\sigma_L{}^2 - sp_0\, q_0}}{sp_0}$$

[1] Arkiv. för Matematik Astronomie och Fysik. Meddelande från Lunds Astronomiska Observatorium. Uppsala and Stockholm. 1912. Band 8, No. 4, p. 31, " Contributions to the Mathematical Theory of Statistics."

See also Yule, " Theory of Statistics," 5th edition, Chap. XIV, Section 10, where the absolute, not the relative, value of σ_p is discussed.

In our case, since q_0 is practically unity, this reduces to $\rho = \frac{\sqrt{\sigma_L^2 - M}}{M}$ where M is the mean. The following table gives the values of 100ρ for all the groups considered, i.e., the percentage variation in the "accident tendency" of the N people involved.

TABLE VI.—*Percentage Variation of the chance of an Accident to different people of the same group. (Charlier's Coefficient of Disturbancy.)*

MEN.				WOMEN.			
Factory.	Group.	No. in Group.	100ϱ	Factory.	Group.	No. in Group.	100ϱ
A ..	I.	204	$92 \pm 5 \cdot 4$[1]	B ..	I.	145	$94 \pm 13 \cdot 3$
A ..	II.	352	$97 \pm 5 \cdot 4$	B ..	II.	100	$88 \pm 10 \cdot 9$
A ..	III.	304	$111 \pm 7 \cdot 5$	C ..	—	58	$95 \pm 14 \cdot 7$
B ..	I.	148	$100 \pm 10 \cdot 8$	D ..	I.	28	$59 \pm 10 \cdot 6$
B ..	II.	299	$88 \pm 14 \cdot 2$	D ..	II.	98	$102 \pm 14 \cdot 8$
E ..	All	3,601	$177 \pm 4 \cdot 7$	G. ..	I.	120	$158 \pm 28 \cdot 3$
E ..	I.	440	$87 \pm 4 \cdot 3$	G ..	II.	50	$139 \pm 43 \cdot 2$
E ..	II.	281	$127 \pm 17 \cdot 4$	H ..	—	346	$94 \pm 10 \cdot 4$
E ..	III.	226	$366 \pm 88 \cdot 4$	I ..	—	161	$103 \pm 17 \cdot 1$
E ..	IV.	256	$132 \pm 21 \cdot 7$	K ..	—	125	$103 \pm 13 \cdot 5$
E ..	V.	77	$149 \pm 21 \cdot 6$	M ..	I.	380	$110 \pm 18 \cdot 2$
E ..	VI.	93	$118 \pm 14 \cdot 5$	M ..	II.	161	$109 \pm 10 \cdot 4$
E ..	VII.	57	$127 \pm 18 \cdot 4$	M ..	III.	142	$79 \pm 12 \cdot 6$
F ..	I.	22	Imaginary	M ..	V.	110	$140 \pm 26 \cdot 2$
F ..	II.	81	$25 \pm 75 \cdot 9$				
G ..	I.	47	$103 + 21 \cdot 2$				
G ..	II.	82	$125 \pm 18 \cdot 9$				
I ..	—	190	$148 \pm 20 \cdot 4$				
M ..	I.	301	$83 \pm 4 \cdot 9$				
M ..	II.	376	$80 \pm 4 \cdot 2$				
M ..	III.	181	$81 \pm 7 \cdot 1$				
M ..	IV.	106	$68 \pm 28 \cdot 8$				
M ..	V.	92	$92 \pm 9 \cdot 8$				
M ..	VI.	218	$117 \pm 10 \cdot 2$				
N ..	—	284	$156 \pm 12 \cdot 2$				

[1] These probable errors are not exact, but found from the approximate formula:—P.E. of ϱ (for the case when $\sigma_B = \sqrt{M}$) $= \frac{.67449.}{\sqrt{N}} \frac{\sigma}{2M^3\varrho} \left\{ (2\sigma^2 - M)^2 + 2M^2\sigma^2 \right\}^{\frac{1}{2}}$.

(For note on this formula, see Appendix III.)

The values of 100ρ are very large, even in the group II of Factory D, which is probably the one in which the conditions were most uniform for all the workers, so that in this case, at any rate, the variation must be very largely personal. The average value of the percentage variation, omitting the obviously unhomogeneous group of the whole Factory E, is, roughly, 100 per cent.

both for men and women, and when the large probable errors are taken into account as well as the even more important factor of the differing scope for the play of variation owing to the small chance of accidents in some groups, the differences from group to group are not excessive. The large size of the variation found here can be compared with that in other types of frequencies given in the following table compiled by Charlier[1] with the last two added from Yule's data[2].

	100ρ
Suicides different years on 5,000,000 ..	20·33
Divorces ,, ,, ,, ..	18·81
Rainy days ,, ,, in Lund	17·65
Drowned ,, ,, on 5,000,000 ..	11·62
Deaths ,, ,, ,, ...	6·99
Marriages ,, ,, ,, ...	5·49
Births ,, ,, ,, ...	4·07
Twins ,, ,, ,, ...	4·17
Births ,, ,, in Lund .. .. (s = 16,000)	3·67
Male infants different districts of Sweden ..	0·95
Male infants different years	imaginary
Deaths in childbirth per 1,000 births (Registration Districts in England and Wales) ..	11 to 21
Male births per 1,000 living births (Registration Districts in England and Wales) ..	0·16 to 1·22 and also imaginary

When we remember that some trouble was taken to try to get departments where the risk of accident appeared to vary as little as possible under normal factory conditions, these figures show how unhomogeneous in this respect even the best of our groups are, and how inadequate the mean number of accidents per person is by itself as a measure of the accident risk in a department.

An increasing number of factories are now keeping minor accident records and making periodical analyses of them (it is surprising incidentally to find even among those whose accident records are fairly detailed, how few refer them to the numbers employed). Such analyses, especially when they give details of the cause and type of accident, are of the greatest value in accident prevention. The distributions obtained in this enquiry suggest that a further step (which involves very little extra arithmetic) might be taken in such periodical summaries, in cases where the works can be divided into fairly homogeneous departments. The average number of accidents per person in any given department is usually calculated as a basis of comparison; if we want to find how to lower this number the first question that arises is, is the average due to conditions arising from the work

[1] *loc. cit.* p. 31.
[2] *loc. cit.* pp. 263 and 283.

or environment which on the whole affect in more or less the same degree all the workers in the department, or is it on the other hand largely affected by a small group of people having many accidents and exposed to special risk, whether such risk arises from personal qualities or individual differences in their conditions of work? Clearly these two cases call for different remedies. A rough approximation to an answer can quickly be obtained by finding the percentage of people in the department who have no accidents, and from the following table (Table VII) seeing what the mean number of accidents per person should be if all were exposed to the same risk:—

TABLE VII.—*Table for a rough determination of the existence of inequality of Accident risk in a Department.*

Percentage of Persons having no Accidents. $100e^{-m}$	Corresponding Mean No. of Accidents per person on "Equal Risk" Theory. m	Percentage of Persons having no Accidents. $100e^{-m}$	Corresponding Mean No. of Accidents per person on "Equal Risk" Theory. m
90·0	·1054	2·25	3·7942
85·0	·1625	2·00	3·9120
80·0	·2231	1·80	4·0174
75·0	·2877	1·60	4·1352
70·0	·3567	1·40	4·2687
65·0	·4308	1·30	4·3428
60·0	·5108	1·20	4·4228
55·0	·5978	1·10	4·5099
50·0	·6931	1·00	4·6052
45·0	·7985	·90	4·7105
40·0	·9163	·80	4·8283
35·0	1·0498	·70	4·9618
30·0	1·2040	·60	5·1160
27·5	1·2910	·50	5·2983
25·0	1·3863	·40	5·5214
22·5	1·4917	·35	5·6550
20·0	1·6094	·30	5·8091
17·5	1·7430	·275	5·8962
15·0	1·8971	·250	5·9915
12·5	2·0794	·225	6·0968
10·0	2·3026	·200	6·2146
9·0	2·4079	·180	6·3200
8·0	2·5257	·160	6·4378
7·0	2·6593	·140	6·5713
6·0	2·8134	·120	6·7254
5·0	2·9957	·100	6·9078
4·5	3·1011	·090	7·0131
4·0	3·2189	·080	7·1309
3·5	3·3524	·070	7·2644
3·0	3·5066	·060	7·4186
2·75	3·5936	·050	7·6009
2·50	3·6889	·040	7·8240

If it is found, as in most of the groups here examined, th. the observed mean is greater than the theoretical mean obtained from this table, then it is probable that the number of accidents in the department is unduly affected by the few people who have many accidents, and observations and experiment among these few may result in finding where the cause is. For instance, in Factory A, Group II, the percentage of men having no accidents is 24·43, this gives an expected mean of about 1·4 accidents per person, the observed mean in this group is 3·78, which is therefore considerably higher[1] than it should be judged by the number who escape accident altogether. (The broken line curve for this group (see Graph I) also shows that theoretically we should not expect any individual to have more than 6 accidents, while, in fact, 68 of them exceed this number and one man has as many as 34 in the six months' period observed. This limiting number, however, depends on the number observed and is in any case not a good criterion to take, being subject to a large probable error.)

If on the other hand the observed mean is not higher than that deduced from the zero group, then it is probable that the accidents that occur are not so much due to individual differences either of work or temperament, but that the causes lie in the general type of work or environment which is common to all the workers in the group, and the remedy lies in looking to these conditions. It is perhaps not unnecessary to repeat the warning given before about the difficulty of distinguishing between a real and an apparent high or low average. It is often the case that diligence in reporting accidents varies in different departments, but it lies in the hands of the factory officials to keep a high standard in this respect. Immediate reporting is in some places encouraged by compensation awards above those legally due, and by penalties for neglect to report even slight injuries.

4. Relation of Accidents to Age and Experience.

It is the common experience in analyses of accident records to find that with the taking on of new men accident rates go up. As these times usually coincide with increased business activity, we find the higher rate sometimes attributed to the speeding up of output and sometimes to the inexperience of the new comers, and it is often difficult from the published records to distinguish between these two factors. The apparent effect of inexperience is well brought out in the following rates for accidents causing loss of time in a large steel plant in America, January–May, 1916. (Chaney and Hanna, 1918).[2]

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[1] An approximation to the probable error of the difference is $\frac{\cdot 67449}{\sqrt{N}}\left(\frac{Q}{P}-m\right)^{\frac{1}{2}}$, where m is the observed mean, P the percentage in the zero group and Q = 100 − P (see Appendix 4).

[2] Bulletin of the United States Bureau of Labour Statistics, No. 234, "The Safety Movement in the Iron and Steel Industry, 1907 to 1917." L. W. Chaney and H. S. Hanna, 1918.

Length of Service.	*Acc. frequency rates per* 1,000 300-*day workers.*	*No. of* 300-*day workers.*
6 months and under	111·3	512
Over 6 months and not over 1 year	104·3	278
Over 1 year and not over 3 years	86·8	357
Over 3 years and not over 5 years	42·4	637
Over 5 years and not over 10 years	19·7	814
Over 10 years and not over 15 years	8·5	470
Over 15 years	—	459
Total	46·5	3,527

Similarly in a motor car factory and a fuse factory covering together 50,000 workers (Frederick S. Lee and others)[1] it was found that the level of the accident rate varied inversely with experience of the workers, and though some of the methods in this report are open to criticism, this finding is in agreement with general experience. In such analyses however, when, as is usually the case, only one factor is considered at a time the interpretation is doubtful.

Experience and age are naturally closely bound together, and it is likewise generally found that the older workers have as a rule fewer accidents than the younger ones when conditions of work are more or less alike (e.g., Chaney and Hanna, *loc. cit.*, also Amy Hewes and Others (1921), who in a study of accidents in a silk mill in Connecticut found that the accident rate decreased with age, that those under 20 had the highest rate, and also that the younger people tended to have more than one injury to a greater extent than the older ones). How far this tendency is due to general immaturity and how far to unfamiliarity with the particular job is not evident without a more detailed analysis. In our present study we have tried to choose periods in which there were no great changes as regards pressure of work, and by breaking up into small groups to make the work in each more homogeneous than is possible in the analyses which cover much larger numbers. Some attempt has also been made to separate the two factors of age and experience by using the method of partial correlation. We have only been partly successful in this as our measure of experience is limited to that of the particular occupation at the period of observation, and consists of the length of service in that particular factory, which in the great majority of cases means service in the same department; where this differed and the information was available, service in the department was taken. Experience in the same occupation but in other factories is thus left out of account.

[1] Public Health Bulletin, No. 106. Washington. "Comparison of an Eight-hour Plant and a Ten-hour Plant."

TABLE IX.—*Experience (Length of Time in Factory in Years).*

Factory.	Group.	Mean.	Standard Deviation.	Coefficient of Variation.
		Males.		
A ..	I.	2·49	1·75	70·3
A ..	II.	2·40	1·71	71·3
A ..	III.	3·37	2·21	65·6
B ..	I.	9·09	10·80	118·8
B ..	II.	10·34	11·80	114·1
F ..	I.	1·15	·96	83·1
F ..	II.	2·28	2·88	126·5
G ..	I.	13·27	13·52	101·9
G ..	II.(*b*)	7·75	7·94	102·5
I ..	—	·86	·88	103·1
M ..	I.	6·78	5·23	77·2
M ..	II.	6·99	4·32	61·8
M ..	III.	8·86	7·11	80·3
M ..	IV.	6·51	5·22	80·2
M ..	V.	15·39	9·49	61·6
M ..	VI.	8·68	5·80	66·8
N ..	—	4·59	6·95	151·3
		Females.		
B ..	I.	4·25	2·48	58·4
B ..	II.	3·53	2·22	62·8
C ..	—	1·67	2·19	131·0
D ..	I.	1·82	1·61	88·5
D ..	II.	1·23	·78	63·7
G ..	I.	4·36	4·73	108·5
G ..	II.	5·60	6·77	120·9
H ..	—	2·45	2·78	113·7
I ..	—	·90	1·06	117·3
K ..	—	3·76	4·96	131·9
M ..	I.	7·63	8·46	110·9
M ..	II.	3·43	4·05	118·2
M ..	III.	4·37	5·18	118·6
M ..	V.	1·32	2·12	159·7

Factory E), or greater numbers of workers (as in Factory A). The groups where the observed values follow the straight line most closely are, on the whole, the larger ones. Men and women both show the same tendency, the women to a less marked degree owing to the smaller numbers of accidents in their case, the only exception being in Factory D, Group II, of women, where the accidents rise slightly with age; but, as we see in considering the correlation coefficients, this is not an exception of any importance, it is also a group in which the variation in age is very small, almost all the girls being under 18.

In a few cases (e.g., A.III, G.I, and N among the men, and G.II among the women), there is some tendency for the workers over 60 to show a slight rise. The numbers at these higher ages

are few, too few for the rise to be of any significance alone, and the point would not be worth mentioning, if it were not in agreement with the course of the industrial accident death-rate discussed below. The suggestion of a parabolic regression is there in these few cases, but on the whole the only tendency that can be said to be definitely established is that for the younger workers to have more accidents than the older ones.

With only two exceptions among the women, (D.II and M.II), the rate of accidents also goes down with both men and women with length of time in the factory, though not so decidedly as with age. It has not been thought necessary to reproduce these graphs.

A more exact idea of the force of these tendencies is given by correlation coefficients[1] of age with accidents and length of service with accidents (Tables X and XI). The distributions of the total coefficients are :—

Correlation Coefficients of Accidents with (a) Age, (b) Length of Service.

	(a) Age.		(b) Length of Service.	
	Males.	Females.	Males.	Females.
+·2 to +·1	0	0	1	0
+·1 „ 0	0	1	0	2
0 „ —·1	3	6	7	5
—·1 „ —·2	9	4	8	6
—·2 „ —·3	5	2	1	0
—·3 „ —·4	3	1	0	1
—·4 „ —·5	3	0	0	0
—·5 „ —·6	1	0	0	0
	24	14	17	14

Age and accidents have a coefficient with its commonest value somewhere near —·2, for both men and women, which agrees with that found by Greenwood and Woods (—·19 and —·18) for women munition workers. In groups where the liability to

[1] In working all these correlations it was necessary to allow for the fact that some of the workers were not exposed to risk for quite all the period. The groups could not all be treated exactly alike. Where the numbers were large those who entered or left during the period were omitted, and in smaller groups the time of exposure to risk was kept constant. Temporary absence from sickness or other causes was treated in a similar way. In some cases no complete record of absence could be obtained; but where possible detailed information was obtained for a sample, and in no case did it appear that the correlation between the number of accidents and the time lost was high enough to make appreciable difference in the other results (e.g., the value of r for the 388 men in E whose names began with B was $\cdot003 \pm \cdot034$, and for a similar sample of 98 in A, $r = -\cdot035 \pm \cdot068$, in fact in more than one case the mean number of accidents was slightly raised by putting in workers who had not the full exposure.

TABLE X.—*Correlation Coefficients of Accidents and Age.*

MALES.

Factory.	Group.	Correlation between number of accidents and age. Total.	Partials, keeping constant— No of days exposed to risk.	No. of days absent sick.	Experience.	Sickness A.	Experience and Sickness A.	Experience and No. of days exposed to risk.	Experience and No of days absent sick.
A ..	I.	—·104±·047	—	—	—·072±·047	—·076±·047	—·040±·047	—	—
A ..	II.	—·253±·034	—	—	—·269±·033	—·226±·034	—·243±·034	—	—
A ..	III.	—·185±·037	—	—	—·180±·038	—·166±·038	—·142±·038	—	—
B ..	I.	—·192±·053	—	—	—·093±·055	—	—	—	—·089±·055
B ..	II.	—·283±·036	—	—	—·220±·037	—	—	—	—·219±·037
E ..	I.	—·271±·030	—	—	—	—·306±·029	—	—	—
E ..	II.	—·114±·040	—	—	—	—	—	—	—
E ..	IV.	—·081±·042	—	—	—	—	—	—	—
E ..	V.	—·450±·061	—	—	—	—	—	—	—
E ..	VI.	—·499±·053	—	—	—	—	—	—	—
E ..	VII.	—·377±·077	—	—	—	—	—	—	—
F ..	I.	—·045±·154	—	—·045±·154	—·100±·153	—	—	—	—·100±·153
F ..	II.	—·143±·073	—	—·144±·073	—·125±·074	—	—	—	—·124±·074
G ..	I.	—·555±·068	—	—	—·514±·072	—	—	—	—
G ..	II.(a)[1]	—·241±·072	—	—	—·239±·073	—	—	—	—
G ..	II.(b)[2]	—·386±·063	—	—	—·364±·065	—	—	—	—
I ..	—	—·309±·044	—·329±·044	—	—·306±·044	—	—	—·328±·044	—
M ..	I.	—·157±·038	—·155±·038	—	—·080±·039	—	—	—·077±·039	—
M ..	II.	—·102±·034	—·118±·034	—	—·090±·035	—	—	—·105±·034	—
M ..	III.	—·154±·049	—·161±·049	—	—	—	—	—·148±·049	—
M ..	IV.	—·063±·065	—·111±·065	—	—·053±·065	—	—	—·102±·065	—
M ..	V.	—·192±·068	—·211±·067	—	—·167±·068	—	—	—·182±·068	—
M ..	VI.	—·225±·043	—	—	—·183±·044	—	—	—·179±·044	—
N ..	—	—·442±·032	—	—	—·411±·033	—	—	—	—

[1] (*a*) excludes and [2] (*b*) includes a group of 5 sole workers with a higher accident average than other workers.

TABLE X.—*Correlation Coefficients of Accidents and Age—contd.*

FEMALES.

Factory.	Group.	Correlation between number of accidents and age. Total.	Partials, keeping constant— No. of days exposed to risk.	No. of days absent sick.	Experience.	Sickness A.	Experience and Sickness A.	Experience and No of days exposed to risk.	Experience and No of days absent sick.
B ..	I.	—·075±·056	—	—	·058±·056	—	—	—	·070±·056
B ..	II.	—·223±·064	—	—	—·012±·067	—	—	—	—·021±·067
C ..	—	—·344±·078	—·382±·076	—	—	—	—	—·367±·077	—
D ..	I.	—·189±·130	—	—	—·172±·131	—	—	—·168±·131	—
D ..	II.	·025±·068	—	—	—·028±·068	—	—	·024±·068	—
G ..	I.	—·272±·057	—	—	—·212±·059	—	—	—	—
G ..	II.	—·193±·092	—	—	—·174±·093	—	—	—	—
H ..	—	—·040±·036	—·068±·036	—	—·032±·036	—	—	—·027±·036	—
I ..	—	—·125±·052	—·128±·052	—	—·101±·053	—	—	—·107±·053	—
K ..	—	—·146±·059	—·180±·058	—	—	—	—	—·110±·060	—
M ..	I.	—·087±·034	—·080±·034	—	—	—	—	·035±·035	—
M ..	II.	—·006±·053	—·039±·053	—	—·131±·052	—	—	—·124±·052	—
M ..	III.	—·048±·057	—	—	·080±·056	—	—	·053±·056	—
M ..	V.	—·081±·064	—·256±·060	—	·060±·064	—	—	—·036±·064	—

TABLE XI.—*Correlation Coefficients—Accidents and Experience—(Length of Service).*

MEN.

Factory.	Group.	Total.	Partials, keeping constant. No. of days exposed to risk.	Age.	Sickness A.	Age and Sickness A.	Age and No. of days exposed to risk.	Age and No. of days absent sick.
A ..	I.	—·092 ± ·047	—	—·054 ± ·047	—·094 ± ·047	—·069 ± ·047	—	—
A ..	II.	—·030 ± ·036	—	·099 ± ·036	—·207 ± ·034	·094 ± ·036	—	—
A ..	III.	—·061 ± ·039	—	·043 ± ·039	—·087 ± ·038	·003 ± ·039	—	—
B ..	I.	—·194 ± ·053	—	—·096 ± ·055	—	—	—	—·098 ± ·056
B ..	II.	—·183 ± ·038	—	·019 ± ·039	—	—	—	·019 ± ·039
F ..	I.	·197 ± ·149	·194 ± ·149	—	—	—	—	·200 ± ·149
F ..	II.	—·102 ± ·074	—·103 ± ·074	—	—	—	—	—·071 ± ·075
G ..	I.	—·259 ± ·092	—	·090 ± ·097	—	—	—	—
G ..	II.	(a) —·043 ± ·077	—	(a) ·024 ± ·077	—	—	—	—
	—	(b) —·137 ± ·073	—	(b) —·014 ± ·074	—	—	—	—
I ..	—	—·042 ± ·049	—·027 ± ·049	—·003 ± ·049	—	—	·012 ± ·049	—
M ..	I.	—·137 ± ·038	—	·018 ± ·039	—	—	·016 ± ·039	—
M ..	II.	—·112 ± ·034	—·137 ± ·034	—·101 ± ·034	—	—	—·126 ± ·034	—
M ..	III.	—·079 ± ·050	—·082 ± ·050	·048 ± ·050	—	—	·050 ± ·050	—
M ..	IV.	—·035 ± ·065	—·044 ± ·065	—·010 ± ·066	—	—	—	—
M ..	V.	—·098 ± ·070	—·110 ± ·070	·019 ± ·070	—	—	·016 ± ·070	—
M ..	VI.	—·164 ± ·044	—	—·096 ± ·045	—	—	—·110 ± ·045	—
N ..	—	—·181 ± ·039	—	·026 ± ·040	—	—	—	—

TABLE XI.—*Correlation Coefficients—Accidents and Experience—(Length of Service).—contd.*

WOMEN.

Factory.	Group.	Total.	Partials, keeping constant.			
			No. of days exposed to risk.	Age.	Age and No. of days exposed to risk.	Age and No. of days absent sick.
B	I.	—·182 ± ·054	—	—·176 ± ·054	—	—·182 ± ·054
B	II.	—·344 ± ·060	—	—·269 ± ·063	—	—·277 ± ·062
C	—	—·062 ± ·088	—·157 ± ·086	—	—·109 ± ·088	—
D	I.	—·091 ± ·134	—	—·045 ± ·135	—·044 ± ·135	—
D	II.	·084 ± ·068	—	·085 ± ·068	—·094 ± ·068	—
G	I.	—·180 ± ·060	—	·045 ± ·061	—	—
G	II.	—·088 ± ·095	—	·028 ± ·095	—	—
H	—	—·024 ± ·036	—·077 ± ·036	·002 ± ·036	—·045 ± ·036	—
I	—	—·115 ± ·053	—·120 ± ·052	—·089 ± ·053	—·098 ± ·053	—
K	—	—·142 ± ·059	—·168 ± ·059	—·078 ± ·060	—·089 ± ·060	—
M	I.	—·109 ± ·034	—	—·074 ± ·034	—·072 ± ·034	—
M	II.	·040 ± ·053	·003 ± ·053	—	·117 ± ·052	—
M	III.	—·074 ± ·056	—	—·098 ± ·056	·070 ± ·056	—
M	V.	—·108 ± ·064	—·262 ± ·060	—·093 ± ·064	—·065 ± ·064	—

accident is greater, the coefficient is naturally higher and rises to $-\cdot 4$ and $-\cdot 5$. Many of the smaller coefficients are not in themselves significant, but the general consistence of the values taken as a whole is in favour of the existence of a real association, though not a strikingly strong one. The connection between length of service and accident is slighter, the most common value of the total coefficient is $-\cdot 1$, and here still more it is rather the whole distribution than the significance of single values that suggest any real association.

To compare the relative effect of age and length of service we pass to the partial coefficients (Tables X and XI), and note that when age is kept constant, the association between length of service and accidents practically vanishes, being as often positive as negative and in almost all cases insignificant. On the other hand, when length of service is kept constant very little appreciable change is made, on the whole, in the relation between accidents and age, and the same is true when allowance is made for the possible disturbing influences of varying length of exposure, etc. The association between accidents and age is therefore the stronger tendency, and has an independent existence, which cannot on these data be shown between accidents and length of service. We must remember, however, that our groups do not contain many absolutely new workers, and it is these, as a rule, who seem to be responsible for the sudden rises in accident-rates with increased trade.

It may be that part of the association between accidents and age is due to selection, i.e. that the younger workers with liability to accident get weeded out, but the fact that the association still remains when length of service is kept constant suggests that this is not a very important factor.

The possibility of a greater willingness to report trivial accidents among the younger workers must be considered, and it may be noted that one of the causes suggested by the Factory Inspectors for the increase in reported (which means more serious) accidents in 1923 over 1922 is "anxiety on the part of elderly men to preserve their employment and full wages, resulting in their concealing trivial injuries until compelled to be off work by sepsis."[1] In our present data, though we cannot hope to have entirely eliminated unreported accidents, we have tried to escape them by careful choice of factories, and one sign that this source of error is not very great here is the fact that all tendencies shown by our figures appear comparatively strongly in Factory A, which had the strictest system of penalties for any unreported trivial accident, and where a full-time Safety First Official made this point his special duty. Another sign is the agreement with the American results quoted above for more serious accidents.

[1] Report of H.M. Chief Inspector of Factories and Workshops for 1923.

In English publications accident-rates at ages are not easy to find.[1] It is a generally accepted opinion that older people are more careful than younger ones, and it may seem superflous to show in such detail that this is supported by the fact that they have fewer accidents, but even a crude numerical measure of any fact, however familiar, is better than no measure at all, and these figures may serve to lay stress on the obvious importance in any potentially dangerous occupation, of giving special definite instruction to young and inexperienced workers and so following the example of the mining industry which is making special efforts in this direction.[2] At the same time, the possibility of a radical psychological difference in the attitude of workers to environments capable of producing minor and serious accidents is not to be overlooked, and this point is discussed later in Section 9.

5.—Accident Mortality Rates at Ages.

If we turn to the mortality at ages from industrial accidents, given in the Registrar-General's supplementary report on "Occupational Mortality," a strikingly different tendency is at once apparent. These show a high *positive* correlation between accident death-rate and age. Collis and Greenwood (1921)[3] have drawn attention to these higher rates at the older ages and illustrated them graphically for 17 large groups from the 1900 to 1902 data. We have compared approximately corresponding groups in the latest report[4] covering the 1910 to 1912 death-rates and find the same general tendency. The course is not really linear, the death-rate often begins with a slight drop, that at 15–20 being rather higher than at the immediately following ages, which then usually show a slow increase ending in a rapid upward rush after about age 55.

In spite of the slight tendency, in a few cases of our data referred to above, for those over 60 to have more trivial accidents, the general courses of the trivial accident rate and the death-rate with respect to age are opposed. Is this inconsistent with the hypothesis that the small accidents we are considering here are really any index to the personal liability to more serious accidents? Not necessarily, as it is possible that relatively more of the old

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[1] See next section.

[2] "To lessen the risks of boys entering the pits an attempt is being made to arrange that they shall be educated in mining dangers in their last year at school, and at evening classes after they have left school. In this the industry has the help and support of the County Mining Education Organisers, who are in touch with the Mines Department." p. 35, Third Annual Report of the Secretary for Mines (for the year 1923), Mines Department.

[3] Health of the Industrial Worker.

[4] "The Mortality of Men in Certain Occupations," Supplement to 75th Annual Report of the Registrar-General.

men die from accidents, not because they really have more accidents, but because they are less likely to recover from them than a younger man would be. We have made a rough test of this with the 17 large occupational groups referred to above. The deaths in each of these groups are given in 8 age classes from 15 years upwards, and the death-rates in these classes were found both for accidents and for all other causes (see Table XII).

To make these observations homogeneous as regards death-rates from one occupation to another, a standardised[1] death-rate was found in each occupation for both accidents and for other causes. Each age group rate both for accidents and for all causes was then expressed as a ratio of the corresponding standardised rate for the same occupation[2]. In spite of the want of strict linearity, a high positive correlation appears between these "corrected" accident death-rates and age, but when the death-rate from other causes is kept constant the correlation between accident death-rates and age becomes negligible, though not actually negative.

Correlation Coefficients. 136 *Observations* (17 × 8).

Age and accident death-rate (corrected) ..	·627 ± ·035
Age and all other causes rate (corrected) ..	·764 ± ·024
Accident and all other causes (corrected) ..	·770 ± ·024
Age and accident death-rate keeping all other causes death-rate constant ..	·093 ± ·057

Hence it appears quite possible that the accident death-rate course may be partly explained rather by the increased "tendency to die" among older people than by an increased "tendency to have accidents."

A perhaps more convincing illustration of this possibility is given by the accident incidence in the well-known Leipsic Insurance Experience.[3] The risk of accident may be so different at different ages, even when the occupations are divided with as much detail as these data allow, that no interpretation can be made of the accident rates themselves at ages without much more knowledge of the working conditions. Out of the twenty-four chief occupations, roughly half show a tendency for the rate to decrease with age and half do not, so that in any case these rates would be inconclusive. They do, however, give us an opportunity for studying the variation with age of the number of compensated accidents to each death by accident (*see* last column of Table XIII).

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[1] The standard population used was that obtained by taking the sums of the actually observed populations at ages in the seventeen groups used.

[2] Or, in other words, the standardised rates were equalised from occupation to occupation.

[3] Krankheits und Sterblickheits Verhältnisse in der Ortskrankenkasse für Leipsic und Umgegend."

TABLE XII.—*The Mortality of Males in Several Occupations, 1910–12. Occupied and Retired Males.*

Rate per 1,000 years of Life. (Uncorrected.)

	Years of Life.	Cause.	AGES.								ALL AGES.	
			15.	20.	25.	35.	45.	55.	65.	75 and over.	Crude.	Standardised.
(49) Iron and steel manufacture; iron, goods makers.	862,977	Accidents ..	·29	·39	·35	·49	·72	1·08	1·60	3·75	·55	·57
		Other causes..	1·84	3·01	4·16	7·40	15·08	32·78	77·84	225·06	12·20	13·14
(52) Nail, bolt, lock, key makers ..	54,765	Accidents ..	—	·16	·09	·33	·38	—	1·31	3·33	·27	·24
		Other causes..	2·19	2·98	3·91	5·57	12·66	27·16	70·00	233·33	14·88	11·73
(45) Engine, machine, boiler-makers, fitters; millwrights.	1,259,322	Accidents ..	·32	·36	·40	·53	·65	·99	1·19	3·88	·54	·55
		Other causes..	1·99	3·31	3·91	6·69	12·98	29·01	74·50	182·14	10·97	11·85
(109) Shoe-makers	579,759	Accidents ..	·14	·17	·11	·20	·32	·37	1·18	3·31	·36	·26
		Other causes..	2·71	4·44	5·27	8·27	14·58	29·50	66·51	182·36	18·18	12·72
(108) Tailors	455,166	Accidents ..	·12	·09	·11	·19	·45	·43	1·17	1·73	·28	·25
		Other causes..	1·71	3·43	4·34	7·65	14·86	30·43	68·49	169·44	14·63	12·15
(117) Bakers, confectioners	373,368	Accidents ..	·21	·17	·15	·19	·35	·35	·55	2·13	·25	·25
		Other causes..	1·74	2·67	3·26	5·92	12·49	26·74	59·22	170·70	10·56	10·46
(113) Butchers	383,619	Accidents ..	·25	·10	·20	·26	·55	·69	1·15	2·78	·33	·36
		Other causes..	1·27	2·56	3·99	7·83	17·51	35·18	67·69	169·82	11·23	12·66
(94) Printers	343,359	Accidents .	·14	·12	·11	·25	·51	·54	1·19	3·17	·27	·31
		Other causes..	2·13	4·30	4·60	7·63	14·06	27·93	64·84	162·37	10·07	11·87
Group of labourers	3,008,970	Accidents ..	·47	·72	·88	1·11	1·44	1·74	2·25	4·92	1·18	1·08
		Other causes..	2·26	5·80	7·77	12·98	22·77	43·02	93·90	281·69	22·98	18·67
Social Class VII (Miners)	2,700,951	Accidents ..	1·32	1·31	1·39	1·57	1·83	2·23	2·25	2·81	1·54	1·58
		Other causes .	1·83	2·56	3·08	5·37	11·14	28·14	80·04	218·11	9·14	11·48
(36g) Coal miners (Monmouthshire and South Wales).	623,118	Accidents ..	1·34	1·33	1·37	1·84	1·94	3·03	3·42	2·56	1·65	1·76
		Other causes..	1·42	2·26	2·89	5·26	11·71	30·24	81·10	186·09	7·53	11·30
(36) Coal miners	2,582,493	Accidents ..	1·32	1·30	1·39	1·57	1·86	2·29	2·30	2·88	1·55	1·59
		Other causes..	1·85	2·53	3·01	5·13	10·79	27·77	79·98	218·89	8·82	11·34
(27) Seamen, etc., merchant service ..	325,047	Accidents ..	3·22	2·76	2·49	2·87	3·07	3·09	1·93	2·96	2·77	2·82
		Other causes..	3·76	6·52	9·72	13·52	23·94	41·98	82·16	169·18	22·98	18·13
(29) Dock labourers, wharf labourers ..	315,999	Accidents ..	·46	·90	1·03	1·29	1·72	2·25	1·91	6·22	1·41	1·26
		Other causes..	1·38	3·63	6·60	11·17	20·27	34·19	64·67	163·56	16·60	14·27
(21 and 22) Platelayers, gangers, packers, and railway labourers.	238,974	Accidents ..	·48	1·07	1·08	1·05	2·30	2·20	2·07	3·02	1·46	1·30
		Other causes..	3·55	3·12	3·58	6·24	10·47	24·50	75·77	242·03	13·91	11·78
(98) Wool, worsted manufacture ..	261,720	Accidents ..	·19	·06	·15	·20	·26	·40	1·27	3·06	·28	·26
		Other causes..	2·57	3·57	4·38	6·87	14·02	33·83	86·23	254·50	15·14	13·81
(92 and 93) Stationery, envelopes, card-board box, etc., manufacture, and dealers in paper prints, books and stationery.	166,029	Accidents ..	·08	·10	·15	·21	·30	·20	·74	22·58	·22	·22
		Other causes..	2·28	4·47	4·54	6·35	10·98	24·08	52·37	164·09	11·04	10·43
Standard population used			2,139,846	1,916,898	3,601,617	2,974,539	2,007,972	1,197,945	557,865	138,954	14,535,636	

TABLE XIII.—*Accident Rates at Ages.*

Rate per 100,000 *male compulsory members observed for one year.*[1]

Leipsic Experience.

Age. Class.	*Rate of Accidents of all Kinds.*	*Rate of Deaths from Accidents.*	*No. of Accidents to one Death.*
Under 15	14,319	17	842
15–24	10,173	34	299
25–34	8,629	41	211
35–44	10,022	72	139
45–54	10,527	98	107
55–64	10,269	117	88
65–74	9,789	233	42
75 and over	6,885	313	22

Total actual number of accidents = 921,862.

Another example is given by Belgian[2] figures for compensated accidents, though the differences between the ages are not so great here as only accidents causing at least twenty-nine days incapacity are included.[3]

TABLE XIV.—*Compensated Accidents in Belgium,* 1912 *and* 1913.

Age Class.	Accidents giving Temporary Incapacity for 29 Days at least. (a)		Accidents giving Permanent Incapacity. (b)		Number of Deaths from Accidents. (c)		Number of Accidents to 1 Death. $\frac{a+b}{c}$	
	1912.	1913.	1912.	1913.	1912.	1913.	1912.	1913.
Under 15	654	713	157	163	18	14	45·0	62·5
15–20	2,164	2,141	452	516	70	67	37·3	39·7
20–30	3,295	3,295	793	832	119	117	34·4	35·3
30–40	3,171	3,181	844	745	119	123	33·8	31·9
40–45	1,373	1,376	320	336	49	48	35·0	35·7
45–50	1,161	1,132	285	276	57	51	25·4	27·6
50–55	934	919	214	225	45	37	25·5	31·0
55–60	676	656	169	152	25	41	33·8	19·7
60–70	583	621	156	151	34	38	21·7	20·3
70 and over	90	86	26	26	7	10	16·6	11·2

[1] The male voluntary members and the women do not give enough deaths for a reliable comparison in their case.

[2] Annuaire Statistique de la Belgique for the year 1922 (published in 1924).

[3] The practical difficulties of making any comparisons between different sets of industrial accident statistics are obvious. A useful summary of methods of definition, classification and compilation in different countries of the various frequency and severity rates in use, and of various schemes suggested for standardisation of accident statistics will be found in "Methods of Statistics of Industrial Accidents." International Labour Office, Geneva, 1923. The standardised system of records recommended by the National "Safety First" Association is described in their Pamphlet No. 2, "Accident Statistics. A Standardised System."

These figures show quite clearly that the variation of the accident death-rate with age is no guide to the course of the accident incidence-rate with age, and it seems probable that the less serious the accidents included, the greater would be the difference between old and young in the number of accidents to one death. The number of days' incapacity from accident per year is an equally unreliable index of the actual numbers of accidents at different ages, and so of the accident tendency. Dr. Vernon[1] found that in different classes of iron and steel workers the number of days' absence per year for compensated injuries increased with age, but it is as true of accidents as of illness that the average duration of a single case increases with age—the following figures from the Leipsic Experience quoted above illustrate this :—

TABLE XV.

Average Duration of Results of Injuries in Days. Leipsic Experience.

Age Class.	Men.		Women.	
	Compulsory Members.	Voluntary Members.	Compulsory Members.	Voluntary Members.
Under 15 ..	16·7	22·7	17·3	11·7
15–24 ..	15·6	21·4	17·9	22·7
25–34 ..	17·4	21·3	21·3	26·3
35–44 ..	20·3	24·6	22·9	28·5
45–54 ..	23·9	30·4	24·7	28·8
55–64 ..	27·1	36·7	26·6	33·7
65–74 ..	33·0	44·1	41·5	36·8
75 and over ..	38·6	48·3	47·6	97·0

The same tendency can be illustrated by the Swedish official figures.[2] There is therefore no necessary contradiction between our results of a decreasing rate of trivial accidents with age, and the higher death and invalidity accident rates generally observed among older workers.

6.—Relation of Accidents to Sickness.

That accidents are not independent of the general state of health seems a natural hypothesis. We have tried to get some measure of the association, but here again the inadequacy of our measurable indices is very obvious.

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[1] Fatigue and Efficiency in the Iron and Steel Industry. 1919. Report No. 5. Industrial Fatigue Research Board.

[2] " On voit que le nombre des cas d'invalidité en % detous les accidents progresse toujours avec l'âge." Olycksfall I Arbete År 1921 sveriges officiella statistik.

In some of the factories dealt with, no sickness records of any kind were available, in others we have obtained individual records of time lost owing to sickness, and in places where there was a good ambulance room and where the habit of reporting minor ailments was encouraged, we have used the records of all such cases during the periods of observation. This last[1] is perhaps the better index of the two for our purpose, since the periods are in some cases too short for the time absent from illness to be a reliable measure, and also absence entails less time of exposure to risk of accident, so that correction for the latter would to some extent wipe out also the variable under examination.

Minor sicknesses reported in the ambulance room are generally too vaguely described for any useful classification to be made; as a rule they do not entail absence from work for more than a few minutes to an hour or two, or the remainder of the shift. The most frequent entries are headache, colds, chills, indigestion, faintness, biliousness, abdominal pains, sore throat, toothache, etc., or simply the all-covering label " not well." The mean numbers of such visits per person and their variability in the six groups of men and six of women for which data were available and appeared reliable are given in Table XVI. The variability is naturally very large. Here, even more than with accidents, does the question arise, as to how much of this is " tendency to be sick " or how much " tendency to report sickness." Clearly we cannot give a definite answer and no doubt some cases occur of workers who keep their minor ailments to themselves and are unwilling to leave their work unless obliged to do so. On the whole, however, the impression gained was that the habit of going to the ambulance room for these ailments varied much more from one factory to another or from one department to another than among the workers in any department where the habit spreads naturally, and only those departments were used where it seemed best established.

Our data give a quite definite positive association between the number of accidents an individual has had and the number of visits to the ambulance room for minor sickness. This is shewn graphically by the regression straight lines in Graphs V and VI, and where accidents and sickness always increase together, and numerically by the correlation coefficients in Table XVII. The average value of the correlation for both men and women is of the order ·3, and it is noteworthy that Factory A, where acc dent reporting is strictly kept up to the mark, gives some of the higher values, which suggests that an explanation of the association by the hypothesis that the. " tendencies to report " accidents and sickness go together, is not a complete one, and that there probably is some real association in the incidence. The size of the coefficient obtained depends on the

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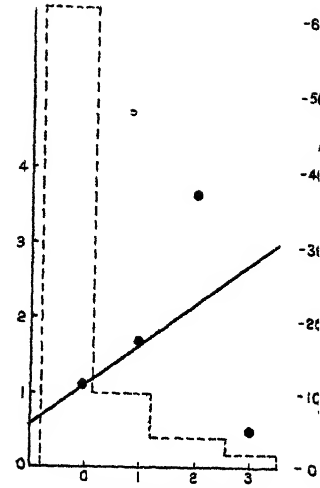
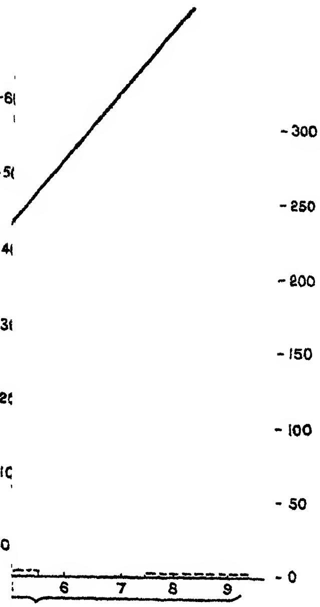
[1] Denoted in the Tables as " Ambulance Room Sickness " or " Sickness A."

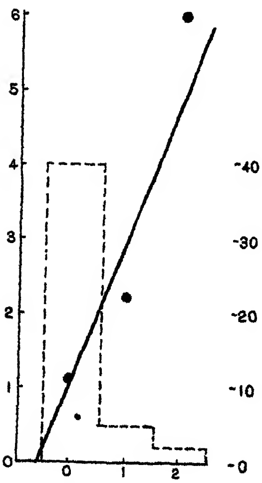
ESSES.



FACTORY G. II.



E. I.



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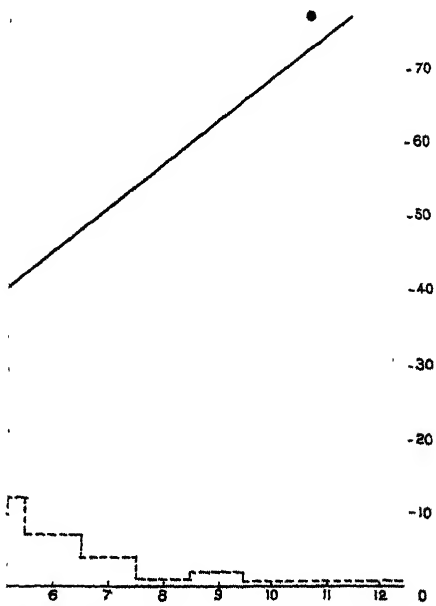
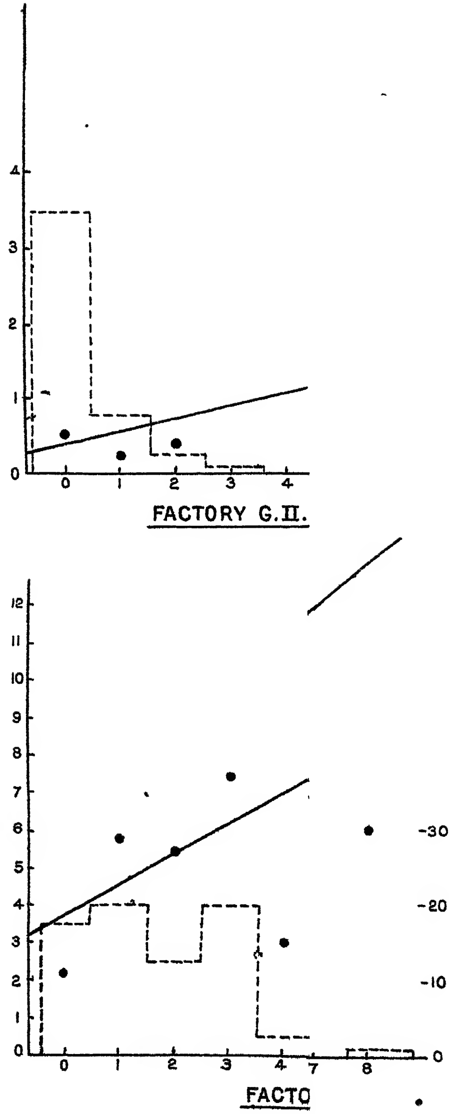


ACTORY A. I.

Horizontal Scale = No: of Sicknes
Vertical Scales {Left Hand = N
Right Hand = No
... No. of Accident



Horizontal Scale *No: of Sick*

Vertical Scales { *Left Hand* / *Right Hand*

● = *Observed No: of Acciden*

by & Sons Lith.

TABLE XVI.—*Sickness A* (i.e., *No. of Ambulance Room Visits, excluding Accidents*).

MEN.							WOMEN.						
Factory.	Group.	Mean No. per person.	Standard Deviation.	Co-efficient of Variation.	C.V. of Poisson with same Mean.	No. of sickness visits per worker per year.	Factory.	Group.	Mean No. per person.	Standard Deviation.	Co-efficient of Variation.	C.V. of Poisson with same Mean.	No. of sickness visits per worker per year.
A ..	I.	2·05	2·32	113·1	69·8	4·10	C ..	—	1·29	1·74	134·7	88·0	3·1
A ..	II.	2·01	2·24	111·3	70·5	4·02	D ..	I.	2·04	1·05	96·01	70·0	1·02
A ..	III.	1·38	1·84	134·1	85·1	2·75	D ..	II.	1·49	2·05	137·7	81·9	·75
E ..	I.	·4659	·9994	214·5	146·5	·233	G ..	I.	·9417	1·46	154·7	103·0	·47
G ..	I.	·1915	·4898	255·8	228·5	·096	G ..	II.	·6	1·18	197·2	129·1	·3
G ..	II.(*b*)	·3171	·6786	214·0	177·6	·159	M ..	I.	·224	·607	271·34	211·3	·28

TABLE XVII.—*Correlation Coefficients—Accidents and Sickness A* (i.e., *No. of Visits to the Ambulance Room*).

MEN.

Factory.	Group.	Total Coefficient.	Partial Coefficient—Keeping Constant. Age	Experience.	No. of days exposed to risk.	Age and experience.	Age and No. of days exposures.	Age, experience and No. of days exposure.
A ..	I.	·418±·039	·412±·039	·418±·039	—	·414±·039	—	—
A ..	II.	·372±·031	·355±·031	·371±·031	—	·354±·031	—	—
A ..	III.	·271±·036	·258±·036	·277±·036	—	·255±·036	—	—
E ..	I.	·246±·030	·285±·030	—	—	—	—	—
G ..	I.	·487±·075	—	·488±·075	—	·385±·084	—	—
G ..	II.	(a) ·189±·074	—	(a) ·187±·074	—	(a) ·128±·076	—	—
		(b) ·137±·073	—	(b) ·127±·073	—	(b) ·037±·074	—	—

TABLE XVII.—*Correlation Coefficients—Accidents and Sickness A.*

WOMEN.

Factory.	Group.	Total Coefficient.	Partial Coefficient—Keeping Constant. Age.	Experience.	No. of days exposed to risk	Age and experience.	Age and No. of days exposed to risk.	Age, experience and No. of days exposed to risk
C ..	—	·382±·076	—	—	·352±·078	—	·290±·081	·272±·082
D ..	I.	·397±·114	—	·388±·115	—	·419±·111	—	·506±·100
D ..	II.	·368±·059	—	·367±·059	—	·366±·059	—	·327±·061
G ..	I.	·311±·056	—	·294±·056	—	·284±·057	—	—
G ..	II.	·188±·092	—	·179±·092	—	·178±·092	—	—
M ..	I.	·200±·033	·193±·033	—	—	—	·194±·033	·188±·033

(*a*) Excludes and (*b*) includes a group of 5 sole workers with a higher accident average than the other workers

scope of variation in the accidents and is larger in departments where more accidents occur (see the order of the three groups of Factory A), so that longer periods would probably have given still higher values.[1]

The suggestion from the figures (and it seems a natural one), is, that so far as these small ailments are a measure of lower general health—and this might be true of the " tendency to report sickness " just as much as of the " tendency to be sick"—the tendency to accidents is associated with such a state. The relation of these small ailments to age or length of service is very slight and only significant in two or three cases (see Tables XVIII and XIX), but any relation there is is an inverse one, i.e., they are rather more frequent among the younger workers. The general experience of illness causing absence is for the actual time lost to increase with age, but the case rates do not always follow the same course, for instance, in an analysis covering five years' sickness experience, the Edison Electric Illuminating Company of Boston[2] found that their case rates decreased progressively with age, and it was especially in the single day absences that the excess of the youngest group was most marked, while in those of over ten days the oldest group led. It may be that part of this is due both here and in our data to a greater readiness to give in to small ailments and to seize an excuse to leave work among the younger workers.

For reasons given above, recorded sickness absence does not seem, in our case, to be a good measure of ill-health, but the figures are given for what they are worth in Table XX,[3] and their coefficients correlation with accidents in Table XXI are on the whole practically negligible.

In order to avoid the fallacy of longer absence from sickness entailing less opportunity for accidents, we have in three Groups I, II and V of Factory M been able to obtain the sickness records

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[1] Both the accident and the sickness distributions were of the J form, with the greater proportion in the zero group in both cases. Although we could find no theoretical justification for the suspicion, we wondered if any of the positive correlation between accidents and minor sickness might be spurious and due to the heaping up in the o o cell owing to the shape of the distributions. We tested this numerically by correlating in Factory A, Group II, the number of accidents suffered by each man with the number of sickness visits to the ambulance room of the ninth man after him in alphabetical order of surname, and the result was $r = - \cdot 028 + \cdot 036$, which showed that the suspicion was unfounded.

[2] *Journal of Industrial Hygiene*, VI, 3 July, 1924. " A Five Years' Sickness and Accident Experience in the Edison Electric Illuminating Company of Boston," by Clarence Oldo Sapington, A.B., M.D.

[3] The warning we have given above against using the figures in Table I to make comparisons *between factories* with regard to accident incidence applies with equal force to the incidence of sickness, whether measured by ambulance room visits or by absence from illness. A high average of sickness absence may be due to exceptional care shown by the firm for the health of its workers, in convalescent benefit schemes or other ways.

TABLE XVIII.—*Correlation Coefficients—Sickness A and Age.*

MEN.					WOMEN.				
			Partial—Keeping Constant.					Partial—Keeping Constant.	
Factory	Group	Total Coefficient.	No. of days exposed to risk.	Experience.	Factory	Group	Total Coefficient.	No. of days exposed to risk.	Experience.
A ..	I.	—·086±·047	—	—·088±·047	C ..	—	—·218±·084	—·242±·083	—
A ..	II.	—·119±·035	—	—·119±·035	D ..	I.	·066±·134	—	+·125±·133
A ..	III.	—·096±·038	—	—·164±·038	D ..	II.	·004±·068	—	—·019±·068
E ..	I.	·098±·032	—	—	G ..	I.	—·155±·060	—	—·084±·061
G ..	I.	—·319±·088	—	—·346±·087	G ..	II.	—·089±·095	—	—·021±·095
G ..	II.	(a)—·295±·070	—	(a)—·284±·071	M ..	I.	—·096±·034	—·097±·034	+·056±·034
		(b)—·271±·069		(b)—·257±·070					

(*a*) Excludes, and (*b*) includes, a group of 5 sole workers with a higher accident average than the other workers.

TABLE XIX.—*Correlation Coefficients—Sickness A and Experience. (Length of Service.)*

MEN.						WOMEN					
Factory	Group.	Total Coefficient.	Partial—Keeping Constant.			Factory.	Group.	Total Coefficient.	Partial—Keeping Constant.		
			Age.	No. of days exposed to risk.	Age and No. of days exposed to risk.				Age	No. of days exposed to risk.	Age and No. of days exposed to risk.
A ..	I.	—·017±·047	·022±·047	—	—	C ..	—	·112±·087	—	·054±·088	·267±·082
A ..	II.	—·029±·036	·028±·036	—	—	D ..	I.	—·203±·129	—	—	—
A ..	III.	·082±·038	·157±·038	—	—	D ..	II.	·035±·068	—	—	—
G ..	I.	—·065±·098	—	—	—	G ..	I.	—·134±·061	—	—	—
G ..	II.	—·086±·076 [1](a) —·088±·074 (b)[1]	—	—	—	G ..	II.	—·126±·094	—	—	—
		—	—	—	—	M ..	I.	—·129±·034	—·102±·034	—	—·103+·034

[1] *See* Note to Table XVII.

TABLE XX.

Absence from Sickness in the Various Groups.

Factory.	Group.	Sickness Absence (No. of Days Absent). Mean No. per Person.	Standard Deviation.	Coefficient of Variation.	Coefficient of Variation by Poisson with same Mean.	Average No. of Days Absent Sick per Person per Year. (approx.)	No. of People affected.	Total No. of People.
					Males.			
A ..	I.	1·37	5·38	392·0	86·0	2·7	43	204
A ..	II.	2·77	6·70	242·0	60·0	5·5	104	352
A ..	III.	3·15	12·04	382·0	56·0	6·3	78	304
B ..	I.	1·47	4·63	314·2	82·5	3·5	37	148
B ..	II.	1·81	7·81	431·9	74·3	4·3	61	299
F ..	I.	1·41	4·52	320·7	84·2	5·6	9	22
F ..	II.	·83	2·19	265·1	110·0	3·3	22	81
M ..	I.(*a*)	7·41	15·38	207·6	36·7	7·4	124	247
M ..	II.(*b*)	11·31	24·82	219·4	29·7	5·7	173	247
M ..	II.(*a*)	4·54	14·18	312·4	46·9	4·5	159	376
M ..	II.(*b*)	8·62	16·55	191·9	34·1	4·3	219	376
M ..	III.(*a*)	6·91	15·65	226·6	38·0	6·9	82	181
M ..	IV.(*a*)	7·20	20·47	284·1	37·3	7·2	40	106
M ..	V.(*a*)	8·77	17·09	194·8	33·8	11·1	43	92
M ..	V.(*b*)	14·62	29·12	199·2	26·2	7·3	55	92
M ..	VI.(*a*)	6·80	16·62	244·4	38·3	6·8	102	218
					Females.			
B ..	I.	1·79	8·43	472·0	75·0	4·3	56	145
B ..	II.	·97	2·03	209·5	101·5	2·3	42	100
C ..	—	3·52	10·67	303·0	53·0	8·4	25	58
D ..	I.	7·59	15·16	200·0	36·0	3·8	15	28
D ..	II.	4·33	7·43	171·7	48·1	2·16	58	98
M ..	I.(*a*)	11·12	22·57	203·0	30·0	14·1	253	380
M ..	II.(*a*)	14·66	21·41	146·1	26·1	18·6	103	161
M ..	III.(*a*)	9·99	17·97	179·8	31·6	12·7	35	142
M ..	V.(*a*)	5·14	11·12	216·3	44·1	7·40	51	110

(*a*) Denotes sickness in 1923, i.e., same period as accident observation period. (*b*) Denotes sickness in 1921 and 1922.

TABLE XXI.—*Correlation Coefficients.—Accidents and Number of Days Absent Sick.*

MEN.

Factory.	Group.	Total.	Partial Coefficient—keeping constant. Age.	Experience.	No. of Days exposed to Risk.	Age and Experience.	No. of Days exposed to Risk and Age.	No. of Days exposed to risk, Age and Experience.
A ..	I.	—·045±·047	—	—	—	—	—	—
A ..	II.	—·078±·036	—	—	—	—	—	—
A ..	III.	—·008±·039	—	—	—	—	—	—
B ..	I.	—·028±·055	—·014±·055	—·037±·055	—	—·026±·055	—	—
B ..	II.	—·012±·039	—·00002±·039	—·014±·039	—	·001±·039	—	—
F ..	II.	·0004±·075	—	·013±·075	—	·001±·075	—	—
M ..	I.(a) (N=301)	·097±·039	·093±·039	—	—	·093±·039	—	·097±·039
M ..	I.(b) (N=247)	·020±·043	·020±·043	—	—	—	—	·074±·043
M ..	II.(a)	·036±·035	·028±·035	—	·070±·035	—	—	·063±·035
M ..	II.(b)	·063±·035	·073±·035	—	·073±·035	·069±·035	—	·081±·035
M ..	III.	—·081±·050	—	—	—·123±·049	—	—	—·100±·050
M ..	IV.	·114±·065	—	—	·129±·064	—	—	
M ..	V.(a)	—·073±·070	—	—	—·046±·070	—	·0001±·070	—·001±·070
M ..	V.(b)	·169±·068	·150±·069	—	·206±·067	·149±·069	·198±·068	·198±·068
M ..	VI.	—·032±·046	—	—·018±·046	—	—	—	—·206±·044

(*a*) Denotes sickness absence in the same period as the accidents (1923).
(*b*) Denotes sickness absence in the two years before the period of observation of accidents (1921 and 1922).

TABLE XXI.—*Correlation Coefficients—Accidents and Number of Days Absent Sick.—contd.*

WOMEN.

Factory.	Group.	Partial Coefficient—Keeping Constant.						
		Total Coefficient.	Age.	Experience.	No. of Days exposed to risk.	Age and Experience.	No. of Days exposed to risk, and Age.	No. of Days exposed to risk, Age and Experience.
B ..	I.	—·097±·056	—·090±·056	—·094±·056	—	—·102±·055	—	—
B ..	II.	—·127±·066	—·151±·066	—·165±·066	—	—·017±·067	—	—
C ..	—	—·075±·088	—	—	—·0014±·089	—	—	—·040±·088
D ..	I.	—·055±·135	—	—·048±·135	—	—·025±·135	—	—·023±·135
D ..	II.	—·020±·068	—	—·026±·068	—	—·023±·068	—	—·021±·068
M ..	I.	—·060±·034	—·054±·034	—	—	—	—·019±·035	—·021±·035
M ..	II.	—·042±·053	—	—	·010±·053	—	·014±·053	·027±·053
M ..	III.	—·103±·056	—·108±·056	—	—	—	—·066±·056	—·058±·056
M ..	V.	—·079±·064	—	—	—·150±·063	—	—	—

for two years previous to the time of observation for accidents ((*b*) in Table XXI). In two groups the correlation is negligible, in the other it is ·206 ± ·067, i.e. small but positive and significant, when the time of exposure for accidents is kept constant, so that this result is quite inconclusive. On these figures, too, no strong tendency is shown for people who are absent much in one period to be also absent in a previous period, but the data are hardly wide enough for any result to be expected.

TABLE XXII.—*Correlation Coefficients between Absence from Sickness in two periods.*

Males.

Factory.	Group.	No. of Days lost by Sickness in 1923 and No. of Days lost by Sickness in 1921 and 1922. Total.	Keeping constant :— Age.
M	I.	·112 ± ·038	·111 ± ·038
M.	II.	·104 ± ·034	·112 ± ·034
M	V.	·0034 ± ·070	—

TABLE XXIII.— *Correlation Coefficients between different Measures of Sickness.*

Factory.	Group.	Total Coefficients. Accidents and No. of times absent Sick.	Sickness A and No. of times absent Sick.	Sickness A and No. of days absent Sick.
		Males.		
A	I.	·098±·047	·442±·038	·259±·044
A	II.	—·006±·036	·420±·030	·314±·032
A	III.	·026±·039	·463±·030	·164±·038
		Females.		
D	I.	—	—	·475±·105
D	II.	—	—	·308±·062
M	I.	—	—	·004±·035

7.—Relation of Accidents to Output, Etc.

Most of the existing studies of accidents in relation to output deal with the secular variations in the mean numbers of accidents in a group of people taken as a whole as the average output varies, and it is usually found that there is a rise in the curve of accidents as the output rises; though this is not always easy to interpret, and is often complicated by the taking on of new workers at such

TABLE XXIV.—*Correlation Coefficients.—Accidents and Output.*

Women.

Factory.	Group.	Total Coefficient.	Partial Coefficient—keeping constant.					
			Time of exposure to Risk.	Time of exposure to Risk and Age.	Time of exposure to Risk, Age and Experience.	Experience.	Age and Experience.	Age, Experience and Time of exposure to Risk.
C	—	·222 ± ·084	·189 ± ·085	·236 ± ·084	·231 ± ·084	—	—	—
D	I.	— ·309 ± ·122	—	—	—	— ·298 ± ·123	— ·249 ± ·127	— ·247 ± ·127
D	II.	·114 ± ·067	—	—	—	·078 ± ·068	·138 ± ·067	·076 ± ·068
(War Data) Munition Makers.	Shift ..	—	—	—	—	—	—	—
(71) Boring ..	Morning ..	— ·098 ± ·079	—	—	—	—	—	—
	Afternoon	— ·209 ± ·076	—	—	—	—	—	—
	Night ..	— ·087 ± ·080	—	—	—	—	—	—
(54) Rough turning	Morning ..	— ·109 ± ·091	—	—	—	—	—	—
	Afternoon	·189 ± ·088	—	—	—	—	—	—
	Night ..	— ·208 ± ·088	—	—	—	—	—	—
(39) Parting off ..	Morning ..	— ·244 ± ·101	—	—	—	—	—	—
	Afternoon	— ·017 ± ·108	—	—	—	—	—	—
	Night ..	— ·049 ± ·108	—	—	—	—	—	—
(43) Groove and Bead.	Morning ..	— ·098 ± ·102	—	—	—	—	—	—
	Afternoon	·053 ± ·103	—	—	—	—	—	—
	Night ..	— ·017 ± ·102	—	—	—	—	—	—

times and by the possible effects of fatigue, etc. Our method here is a different one; we have tried to get an average measure for the output of each individual in the group over the whole period of observation, and compared these figures with the numbers of accidents suffered by that individual. In only three cases (all referring to women) was it possible to get comparable figures for output. Those used are deduced from weekly wages with some necessary corrections for overtime and for increases with age.

We have included some additional figures relating to women munition workers obtained during the war by Miss C. Allen and myself. None of the correlation coefficients are significant, and they are not consistent in sign (though, on the whole, the negative sign prevails), so that no association can be inferred from these figures. In other words, in these samples, which are both few and small, the better workers (so far as this can be judged from their piecework wages) sometimes have more and sometimes fewer accidents than the others.

The only other personal record of any kind generally kept for factory workers is that of punctuality in attendance. This also shows no consistent relation with the incidence of accidents; the correlation co-efficients were all small and not consistent in sign, hence they have not been reproduced.

8. "Corrected" Distribution.

We have seen that much of the individual variation shown in the incidence of accidents is not due to chance, and though differences in work conditions may account for some of this, the associations found with age and minor sicknesses show that *part*, at any rate, of the tendency is personal. If the part that is associated with these two factors can be so definitely demonstrated, it seems probable that other personal factors play a large part too.

In comparing the observed with the theoretical chance distributions, we corrected for differences of risk in the work, as far as was possible, by taking the sum of separate Poisson series for each department; we can now go a step farther by making a rough correction in the observed distribution for age and for "sickness tendency" as measured by ambulance room visits. For this test, Group II of Factory A was chosen, partly for its size (352 males) and partly because the linear relation between the number of accidents and the other two factors was more definite here than in many of the other groups.

By means of the partial regression equation[1], the theoretical value of each man's accidents was found from his age and sickness visits, and the excess of the corresponding observed value over this

[1] The equation is $x_1 = \cdot 6397\, x_2 - \cdot 0961\, x_3 + 5 \cdot 7587$, where x_1 = number of accidents, x_2 = number of sickness visits, x_3 = age in years.

TABLE XXV.—*Distribution of Accident Frequencies "Corrected" for Age and "Sickness Tendency."*

Factory A. Group II. Males.

	(i) Uncorrected.			(ii) Corrected.		
	Observed.	Fitted to Single Poisson.	Fitted to Sum of Poissons by Depts.	Observed	Fitted to Single Poisson.	Fitted to Sum of Poissons by Depts.
0	86	4·9	8·9	116	21·2	23·6
1	36	20·9	32·0	46	59·6	61·5
2	34	44·6	58·1	44	83·7	82·2
3	51	63·7	71·1	31	78·4	75·1
4	35	68·1	66·2	39	55·0	52·9
5	25	58·3	50·0	23	30·9	30·7
6	17	41·6	32·0	13	14·5	15·3
7	17	25·4	17·9	8	5·8	6·7
8	11	13·6	8·9	7	2·0	2·6
9	7	6·5	4·0	9	·6	·9
10	12	2·8	1·7	5	·2	·3
11	4	1·1	·6	3	—	·1
12	4	·4	·2	1	—	—
13	4	·1	·1	2	—	—
14	—	—	—	—	—	—
15	2	—	—	2	—	—
16	2	—	—	1	—	—
17	2	—	—	—	—	—
18	1	—	—	—	—	—
19	—	—	—	—	—	—
20	—	—	—	—	—	—
21	—	—	—	—	—	—
22	—	—	—	1	—	—
23	—	—	—	—	—	—
24	1	—	—	—	—	—
25	—	—	—	—	—	—
26	—	—	—	—	—	—
27	—	—	—	—	—	—
28	—	—	—	—	—	—
29	—	—	—	—	—	—
30	—	—	—	—	—	—
31	—	—	—	1	—	—
32	—	—	—	—	—	—
33	—	—	—	—	—	—
34	1	—	—	—	—	—
χ^2 for 9 groups, grouping all Nos. over 8 accidents.		1,479	878	—	1,140	821

theoretical number was added to (or the defect subtracted from) the mean number of accidents for a man aged 34, who had no sickness visits (34 being the mean age of this group), and the nearest whole number taken for the number of the man's accidents (negative values were all treated as zero, i.e., put in the group

found the correlation between their accidents in two successive five months' periods to be $\cdot61 \pm \cdot02$. We tested this figure for possible spurious correlation due to combining operations by taking four smaller samples from them, each homogeneous in itself, viz. :—

	Correlation Coefficient.
29 women on rough turning	$\cdot71 \pm \cdot06$
17 women on parting off..	$\cdot56 \pm \cdot07$
30 women on groove and bead	$\cdot56 \pm \cdot07$
54 women on boring	$\cdot04 \pm \cdot09$

Another test was made by combining these 150 women, using instead of each woman's actual number the ratio of this to the mean number in her operation and period; the result was $\cdot59 \pm \cdot03$, hence showing that the correlation was not spurious. Coefficients of the same order were found for 39 women on parting off during a six months' period between the accidents in morning, afternoon and night shifts. Two of these groups of munition workers, those on profiling and rough turning, were particularly liable to have accidents from flying particles of hot metal, which caused either eye accidents or burns on the face or other exposed parts of the body. The following correlations between these and other types of accidents[1] among 64 women in two successive periods of seven months and three months show individual susceptibility also in this particular type of accident which might at first sight be thought to be entirely of chance origin. In this particular munition factory great strictness was observed with regard to accident reporting.

Coefficients of correlation between the incidence on the same individuals of—

(1) Accidents due to flying particles in the two periods	$\cdot59 \pm \cdot06$
(2) Other accidents in the two periods ..	$\cdot38 \pm \cdot07$
(3) Accidents from flying particles in period A and other accidents in period B ..	$\cdot56 \pm \cdot06$
(4) Other accidents in period A and accidents from flying particles in period B ..	$\cdot32 \pm \cdot08$

The stability of susceptibility from one period to another is on the whole confirmed by our present data. We have only used for this purpose the groups covering the longer periods of observations.

[1] Correction was made for varying numbers of hours worked.

TABLE XXVI.—*Correlation coefficients between the accidents of the same workers in two periods.*

Factory.	*Groups.*	*Correlation Coefficient.*	*No. of People.*	*Length of periods* (*a*)	(*b*)
D	I Females	$\cdot 21 \pm \cdot 15$	19	2 years	5 months
D	II Females	$\cdot 36 \pm \cdot 09$	42	2 years	5 months
E	I Males	$\cdot 57 \pm \cdot 02$	445	1 year	1 year
E	II Males	$\cdot 25 \pm \cdot 04$	288	1 year	1 year
E	III Males	$\cdot 62 \pm \cdot 03$	226	1 year	1 year
E	IV Males	$\cdot 20 \pm \cdot 04$	288	1 year	1 year
G	I Males	$\cdot 36 \pm \cdot 09$	47	1 year	1 year
G	II Males	$\cdot 57 \pm \cdot 05$	82	1 year	1 year
G	I Females	$\cdot 53 \pm \cdot 04$	120	1 year	1 year
G	II Females	$-\cdot 01 \pm \cdot 10$	50	1 year	1 year
H	Females	$\cdot 05 \pm \cdot 05$	227	6 months	6 months

Only the last two of these groups failed to show positive association, and these are among the females who give smaller values in all our correlation tables owing to the fact that they have on the whole fewer accidents in the periods observed, so that there is less scope for variation. It is of course true that in these data the possibility of spurious correlation due to some amount of varying risk is present and cannot be avoided, but in four groups[1] of Factory M, two of men and two of women, we had an opportunity of testing the stability without this complication. In these groups, some of the workers were treated at the ambulance room for accidents received at home, and the number of any individual's "home accidents" shows a positive correlation with the number of his or her factory accidents, see Table XXVII. The correlation is positive in all four cases and of the order ·2 to ·3.

"Tendency to report" might, of course, play some part here, too, and no measure of this can be obtained. Another possibility that suggested itself for this correlation was that the home accidents might only be treated when the worker happened to be in the ambulance room for some accident received in the factory, but comparison of the dates show that this is not the case. The same people who are unfortunate in the factory seem also to be unfortunate at home. This is only in accordance with ordinary experience, that though there may often be some element of truth in the excuse "the knife slipped" or "the plate came to pieces in my hand," there is usually a much larger element of truth in the statement that there are some people in whose hands knives and plates are more likely to do this than in those of others.

[1] In the other groups of this factory the number of home accidents was not large enough to deal with.

TABLE XXVII.—*Correlation Coefficients between the incidence on the same individual of Accidents in the Factory and Accidents at Home.* (*Factory M.*)

Men.						Women.					
Group.	Total Co-efficient.	Partial Coefficient—Keeping Constant.				Group.	Total Co-efficient.	Partial Coefficient—Keeping Constant.			
		Exposure to Risk.	Age.	Exposure to Risk and Age.	Exposure to Risk, Age and Experience.			Exposure to Risk.	Age.	Exposure to Risk and Age.	Exposure to Risk, Age and Experience.
I.	·200±·037	·199±·037	·170±·038	·170±·038	—	II.	·261±·050	·223±·051	—	·222±·051	·236±·050
II.	·213±·033	·207±·033	·201±·033	·195±·033	—	III.	·311±·051	·307±·051	—	—	—

10.—Minor Accidents as a criterion for Major Accidents.

It may be objected that the trivial accidents dealt with here are no criterion for the incidence of more serious accidents. Since any minor accident, however small, might by a very small alteration in attendant circumstances or subsequent treatment become a major accident, there seems to be some *a priori* grounds for supposing that the individuals who are more liable to small accidents are also more liable to more serious ones. Unfortunately, in the present data the number of more serious accidents (which for present purposes we have defined as accidents causing lost time), is so small in the periods observed that they give no definite statistical evidence on this point, so that, until more figures are available, it must remain mainly a matter of opinion. Mr. Farmer has, however, collected a large amount of material from dockyard accidents on this question which he is now analysing.

11.—Night and Day Work, Etc.

Our primary object in collecting the present data was to study individual differences and throughout the whole of this work we have looked on each group as a unit in itself, and purposely avoided making any direct comparisons from group to group owing to differing conditions from one group to another.

There is, however, one pair of groups (Factory M, Males, Groups I and II) which are directly comparable as they were doing the same work and were observed for the same period; Group I being on the day shift and Group II on night shift, so that they afford a comparison of accidents on day and night work at the same occupation. There is no significant difference between either the mean number of accidents per person or their variability (Mean of Group I $= 3{\cdot}94 \pm {\cdot}14$ and Group II $= 3{\cdot}98 \pm {\cdot}14$, Standard Deviation of Group I $= 3{\cdot}84 \pm {\cdot}11$ and Group II $= 3{\cdot}75 \pm {\cdot}09$).

The set of men on the day shift, were, however, about eight years younger on the average than those on the night shift, though their length of service in the factory was practically the same. If we give the association between age and accidents its mean value for these two groups, we should expect the mean number of accidents to be about ·6 less in the older group, so that what tendency there is, is for a very small increase in accidents during the night, but it is not large enough to be of any importance, so that in this case, the forces tending to higher accident rates at nights are about equally balanced by those tending to lower. The values of the different correlations are also about the same in the two groups.

We have not been able to make comparison between the married and single women, as practically the bulk of women workers were unmarried, and the married and widows were too few in any group to make comparison valid.

The data are not really suitable for studying seasonal variations and were not collected for this object, but in case anything definite of this sort was to be detected, we have arranged the accidents into half-monthly periods according to the calendar month, but they do not show any consistent variations that cannot be accounted for by holidays, etc., so that the results are not reproduced here. Since the seasonal character of sickness in general is a definite and well-established fact, any similar tendency in the incidence of accidents might lead to a group correlation between sickness and accident rates. In the present data, however, the correlations found are individual ones in which all the individuals within a single group were observed over the same period of the year, so that the correlations found within the groups cannot be due to any latent seasonal current in the accidents.

12.—Summary and Conclusions.

(1) The reported ambulance-room accidents (whether trivial or serious) have been examined in a number of groups of factory workers covering different occupations, for periods of time varying from three months to two years.

(2) It is found that in almost all the groups the average number of accidents is much influenced by a comparatively small number of workers, and that the distributions among the workers are far from chance ones.

(3) It is not possible in a mass examination of this kind to find how much of this may be due to individual differences in the conditions of work or how much to personal tendency, but there are many indications that *some* part, at any rate, is due to personal tendency.

(4) A table is given (Table VII) by which a rough estimate can easily be made from the accident records of a department, whether the causes of accidents are mainly such as affect all the workers alike, or whether special risk is attached to any individuals. It the latter is the case, it rests in the hands of the factory officials, familiar with all the conditions of the work, to see how far the cause is mechanical or personal.

(5) Typical accident distributions are described and fitted to some theoretical curves based on different hypotheses. The hypothesis that the occurrence of one accident makes the occurrence of others either more or less likely does not fit these observations. The particular hypothesis tested of differing initial individual susceptibility is nearer the truth, but clearly other factors come in.

(6) There is a tendency for the number of accidents to decrease to some extent with age, and apparently also, though to a less extent with length of service in the factory, but when

allowance is made for age, there is no independent association between experience and accidents ; while when allowance is made for experience, the association between accidents and age remains.

(7) A decreasing accident tendency with age towards serious accidents is shown not to be necessarily inconsistent with the known higher accident mortality and invalidity rates among older workers, but the present data do not include enough serious accidents to establish any relation between the tendencies to trivial and to serious accidents.

(8) The people who have the most accidents are, on the whole, those who pay most visits to the ambulance room for minor sicknesses.

(9) The consistence of individual tendency to accident is shown by the association found between (1) accidents in two different periods, (2) accidents of one type and accidents of other types, and (3) accidents in the factory and accidents at home.

(10) No consistent relation is shown between accidents and output in the few cases where output records were available.

(11) The above tendencies are shown by men and women alike.

(12) In the one group of males, where comparison between night and day work was possible, no difference of any importance was observed in the accidents.

(13) The indications of individual differences seem to be definite enough to justify the further more detailed investigation on the lines of individual study and experimental psychology which is now being carried on, rather than on the lines of the present report. The present results show that it is clearly necessary in such study to take age and health into consideration.

We should again like to acknowledge the obligation we are under to those firms who have co-operated with us in this enquiry, and to thank both the managements for their interest and the facilities given us, and for reading the proofs of the reports which were submitted to them for criticism, and also the various members of the factory staffs who have helped us both by keeping the records and by the suggestions and information they have so readily given.

APPENDIX I.

General Form of the Accident Distributions.

As examples of the general forms of the distribution curves of accidents, the higher moments and the constants β_1 and β_3 are given in Table XXVIII for some of the larger factories and groups. None of these are pure groups, all devoted to one single occupation, but I think they can be taken as fairly representative since it would not be easy from inspection of Graphs I and II to pick out the composite groups from the pure ones.

For Factory A and its groups the crude moments are compared with those corrected by Pearson's "abruptness coefficients." The material does not seem very suitable for the use of these corrections as the fifth order differences are large, and the exactness of the method depends on the fit of a fifth order parabola to the initial frequencies. If this fit is bad, absurd results can be obtained when very small subfrequencies are used. In the case of Factory A, the alterations made by the corrections are unimportant. This is probably due to the fact that no smaller sub-groupings are available for the initial frequencies. Hence, for the other factories, no corrections have been made.

Two of the largest distributions—those for the whole factory, males, in Factories A and E—have been fitted by Pearson's frequency curves. The values of the β's in E are very high ($\beta_1 = 21\cdot84$, $\beta_2 = 38\cdot55$) and hence have very large probable errors, so that the constants are unstable, and in both cases the curves cannot be regarded as anything more than moderately poor smooths of the particular cases. The values of the β's are not such as to bring either of the curves into the heterotypic area; Factory A is a Type I(J), i.e., a J curve of limited range, and Factory E a Type VI(J), i.e., a J curve of unlimited range. If we fit these curves in the usual way by four moments, placing the curve down on the histogram so that the means coincide, then the start, i.e., the infinite ordinate, will not coincide with the left-hand boundary of the first frequency block of the histogram, in both cases the theoretical curve start comes a little to the right of this bounding line; if on the other hand we put the curve down so that the start comes in the right place, the means will not quite coincide, the observed distribution having the higher mean in each case (the difference is $\cdot1751$ accidents in the case of A, and $\cdot6244$ accidents in the case of E). Since it seems reasonable to consider the start of the curve known, and also desirable in fitting by moments to make the means coincide, both curves have also been fitted by making both the means and the starts coincide and using only three moments instead of four. The results of all three methods are given in Tables XXIX and XXX, and the graphs of the third method in the lower figures in Graph VII. The fits are not very good in either case, but considerably better in Factory A than in Factory E. In both cases the third method (using only three moments and putting both mean and start together) is the best as judged by the Goodness of Fit Test for 15 or 14 groups, though in A the value of P is practically identical with that using moments up to the fourth and equating the starts.

Of the two methods of placing the curves when four moments are used, in Factory A we get a better result by superimposing the starts, and in Factory E by superimposing the means. Both from the Goodness of Fit Test and from consideration of the high probable errors of the higher moments the third method seems preferable.

TABLE XXVIII.—*Constants of Distributions of Accidents.*

	Whole Factory A. (Men.)		A. Group I.		A. Group II.		A. Group III.		B. Men (whole factory).	B. Women (whole factory).	E. Men (whole factory).
No. of persons ..	860		204		352		304		447	267	3,601
	Corrected.[1]	Uncorrected.	Corrected.	Uncorrected.	Corrected.	Uncorrected	Corrected.	Uncorrected.			
Mean ..	3·943	3·977	6·411	6·436	3·748	3·778	2·513	2·556	·978	1·41	1·36
Standard Deviation	4·803	4·780	6·492	6·472	4·426	4·142	3·275	3·249	1·61	1·88	2·68
μ_2 ..	23·065	22·853	42·144	41·883	17·330	17·155	10·727	10·556	2·58	3·55	7·18
μ_3 ..	238·526	238·174	391·390	391·800	160·754	160·802	67·222	66·996	13·96	13·79	89·90
μ_4 ..	5133·901	5102·084	9165·510	9114·418	3675·932	3655·847	855·267	844·730	134·48	102·30	1987·03
β_1 ..	4·637	4·753	2·046	2·089	4·966	5·121	3·661	3·816	11·40	4·24	21·84
β_2 ..	9·650	9·769	5·160	5·196	12·240	12·422	7·433	7·581	20·25	8·11	38·55
Total No. of Accidents	3,420	3,420	1,313	1,313	1,330	1,330	777	777	437	376	4,915

[1] Corrected by Pearson's "abruptness coefficients."

TABLE XXIX.—*Factory A (Whole Factory).* Fitted to Pearson's Type I Curve.

No. of Accidents.	*Observed.*		*Calculated "A."*		*Calculated "B."*		*Calculated "C."*	
0	230		231·18		202·49		203·60	
1	105		132·93		141·48		134·66	
2	83		98·48		103·33		102·88	
3	90		76·31		79·64		80·78	
4	71		60·37		62·83		64·26	
5	50		48·33		50·22		51·50	
6	47		38·98		40·46		41·48	
7	44		31·61		32·78		33·53	
8	26		25·72		26·66		27·17	
9	16		20·98		21·74		22·06	
10	25		17·15		17·77		17·94	
11	12	25	14·04	25·55	14·54	26·45	14·61	26·52
12	13		11·51		11·91		11·91	
13	9	18	9·43	17·17	9·77	17·78	9·72	17·65
14	9		7·74		8·01		7·93	
15	3	14	6·35	19·35	6·58	20·05	6·48	19·65
16	6		5·21		5·40		5·30	
17	3		4·28		4·43		4·33	
18	2		3·51		3·64		3·54	
Over 18	16		15·88		16·32		16·34	
Total	860		859·99		860·00		860·02	
Goodness of Fit Test.	χ^2		25·85		33·72		25·82	
(15 groups)	P		·0272		·00489		·0274	

"A" is calculated from the curve—

$$y = 10^{-90} \times 4213{\cdot}6295x^{-{\cdot}244382}(223{\cdot}13167 - x)^{37{\cdot}745936}$$

whose constants are derived from equating the first four moments[1] about the mean, but the curve is put down on the observed histogram so that the start of the curve (i.e., the infinite ordinate) coincides with the beginning of the first frequency of the histogram. The means do not then coincide, but are at a distance ·175067 accidents apart (the observed curve having the higher mean).

"B" is calculated from the same curve whose equation is given above, i.e., using the moments[1] up to the fourth, but the curve is placed on the histogram so that the means coincide. The start of the calculated curve (i.e., the infinite ordinate) is now at a distance ·175067 accidents to the right of the beginning of the first frequency of the histogram.

"C" is calculated from the curve—

$$y = 10^{-1690} \times 2{\cdot}415582x^{-{\cdot}14734345}(2482{\cdot}3841 - x)^{474{\cdot}508446}$$

whose constants are found from making both the starts and the means of the two curves coincide and equating also only the first three moments[1] about the mean.

[1] Moments used have been corrected by abruptness coefficients.

TABLE XXX.—*Factory E (Whole Factory).* Fitted to Pearson's Type VI Curve.

No. of Accidents.	*Observed.*		*Calculated "A."*		*Calculated "B."*		*Calculated "C."*	
0	1888		2609·17		2127·91		1830·35	
1	779		351·64		633·57		705·97	
2	343		191·56		272·21		383·38	
3	187		121·17		159·47		226·54	
4	108		82·27		104·12		141·13	
5	93		58·26		71·98		91·44	
6	51		42·44		51·58		61·14	
7	32		31·57		37·90		41·98	
8	20		23·85		28·36		29·47	
9	27		18·25		21·54		21·41	
10	15	26	14·11	25·12	16·55	29·40	15·38	26·77
11	11		11·01		12·84		11·39	
12	13	20	8·65	15·50	10·05	17·97	8·55	15·05
13	7		6·85		7·92		6·50	
14	5	13	5·45	16·16	6·28	18·55	5·00	14·36
15	5		4·36		5·01		3·89	
16	2		3·51		4·02		3·05	
17	1		2·83		3·24		2·42	
Over 17	14		14·02		26·45		12·02	
Total	3601		3600·98		3601·00		3601·01	
Goodness of Fit Test.	$\chi^2 =$		911·46		102·79		38·99	
(14 groups)	P =		<·0000001		<·0000001		·000598	

" A " is calculated from the curve—

$$y = 10^{38} \times 1\cdot7701969x^{-18\cdot1109301}(x - 90\cdot68177)^{-\cdot769133}$$

whose constants were derived from equating the first four moments[1] about the mean, but the curve is put down on the observed histogram so that the start of the curve (i.e., the infinite ordinate) coincides with the beginning of the first frequency of the histogram. The means do not then coincide but are at a distance ·62437 accidents apart (the observed curve having the higher mean).

" B " is calculated from the same curve whose equation is given above, i.e., using the moments[1] up to the fourth, but the curve is placed on the histogram so that the means coincide. The start of the calculated curve (i.e., the infinite ordinate) is now at a distance of ·62437 accidents to the right of the beginning of the first frequency of the histogram.

" C " is calculated from the curve—

$$y = 10^{9} \times 4\cdot022517x^{-6\cdot103978}(x - 11\cdot113079)^{-\cdot2666664}$$

whose constants were found from making both the starts and the means of the two curves coincide and equating also only the first three moments[1] about the mean.

---

[1] In this case " abruptness " corrections were not used owing to the irregularity of the differences.

APPENDIX II.

Note on the Coefficient of Variation.

In most of the ordinary physical measurements the coefficient of variation is a satisfactory measure of variation in so far as it is an absolute number—independent of the unit of measurement—and so to some extent makes possible the direct comparison of the variability of magnitudes of different dimensions or kinds, such as height, volume, weight, etc. In our present case it has two drawbacks, both due to the imperfections of our measures, and of these the second is the more important.

The first arises from the fact that, supposing the distribution of accidents among the population to be a chance one, if the period of observation were lengthened so that the mean number of accidents per person were doubled, the standard deviation (being $\sqrt{m}$ where m is the mean) would not be doubled, but only multiplied by $\sqrt{2}$, so that the coefficient of variation in the longer period would be less than in the first, although the character of the population and conditions have not been changed. Hence the coefficients of variation in Table I are not comparable from group to group, being functions of the varying periods of observation. Each has to be considered in relation to the theoretical coefficient of variation given by the appropriate Poisson series.

The second drawback does not arise when we look on the figures simply as numbers of accidents, but it does arise when we try to use them as approximate measures of an underlying quality—tendency to accident. Such a quality, in common with a great many other human mental or moral characteristics and some physical ones, has no absolute natural zero, or, at least not one on which we can lay a finger and take as an origin of measurement. According to our more or less arbitrary choice of zero, so will our coefficient of variation differ in the same observations, since change of origin will alter the value of the mean but not of the standard deviation. Temperature is a physical example—the coefficient of variation of a set of observations, whose mean is 98° F. and standard deviation 12° F., is 12·2 in the Fahrenheit scale, 18·2 in the Centigrade scale, and if absolute zero is taken, 2·2 in either Fahrenheit or Centigrade.

Most, or perhaps all, mental and performance tests, examination results, or indeed any of the various more or less inadequate measures, to which for want of something better we are at present limited in estimating human characteristics of a psychological nature, suffer from the fact that their zero is an arbitrary one imposed by the nature of the test, and does not in fact necessarily imply entire absence of the quality in question. Even the most ardent supporter of "Mental Tests," or the most hardened examiner, would hardly maintain that a zero mark always indicates complete imbecility. A measure of the variability as well as of the average value of such qualities is often needed, and it is of importance to remember that in such cases, not only does the absolute value of the mean as usual depend on the unit of measurement, but that the coefficient of variability has not such a definitely real meaning here as in many physical measurements, but can only be interpreted in relation to the particular zero adopted.

In the present case it is quite obvious that though the actual occurrence of a reported accident in the period watched is the smallest unit practically available, the people who have no accidents are probably of very varying grades of aptness or carefulness, and that the "margin of safety" among them, whether mechanical or personal, may differ greatly from man to man.

APPENDIX III.

Approximation to the Probable Error of Charlier's Coefficient of Disturbancy, for the case when $\sigma_B = \sqrt{M}$.

$\varrho = \dfrac{\sqrt{\sigma^2 - sp_0 q_0}}{sp_0}$ where the symbols have the meanings explained on pp. 21 *et seq.*

When, as in our case, $\sigma_B = \sqrt{M}$, so that $sp_0q_0 = sp_0 = M$ approx:

$$\varrho = \frac{\sqrt{\sigma^2 - M}}{M}$$

$$\therefore \log \varrho = \tfrac{1}{2} \log (\sigma^2 - M) - \log M$$

$$\therefore \frac{\delta\varrho}{\varrho} = \tfrac{1}{2} \frac{2\delta\sigma\sigma - \delta M}{\sigma^2 - M} - \frac{\delta M}{M} \text{ to a first approximation*[1]}$$

$$\therefore \delta\varrho = \frac{\varrho}{2M(\sigma^2 - M)} (2M\sigma\delta\sigma - M\delta M - 2\sigma^2\delta M + 2M\delta M)$$

$$= \frac{\varrho}{2M(\sigma^2 - M)} \left\{ 2M\sigma\delta\sigma - (2\sigma^2 - M)\,\delta M \right\}$$

$$\therefore \delta\varrho^2 = \frac{\varrho^2}{4M^2(\sigma^2 - M)^2} \left\{ 4M^2\,\sigma^2\,\delta\sigma^2 + (2\sigma^2 - M)^2\,\delta M^2 \right\} \text{ if we assume}$$

that there is no correlation between the mean and standard deviation.*[2]

Taking mean values and putting $\frac{\sigma^2}{2N}$ for σ_{σ^2} and $\frac{\sigma^2}{N}$ for σ_{M^2}

$$\sigma\varrho^2 = \frac{\varrho^2}{4M^2(\sigma^2 - M)^2} \left\{ \frac{4M^2\,\sigma^4}{2N} + \frac{(2\sigma^2 - M)^2\,\sigma^2}{N} \right\}$$

$$\therefore \sigma_\rho = \frac{\varrho\sigma}{2M(\sigma^2 - M)} \frac{1}{\sqrt{N}} \left\{ 2M^2\,\sigma^2 + (2\sigma^2 - M)^2 \right\}^{\frac{1}{2}}$$

But $\sigma^2 - M = M^2\varrho^2$

$$\therefore \sigma_\varrho = \frac{\sigma}{2M^3\varrho} \cdot \frac{1}{\sqrt{N}} \left\{ 2M^2\sigma^2 + (2\sigma^2 - M)^2 \right\}^{\frac{1}{2}} \text{ approx.}$$

---

*[1] It is probable that this approximation is not justified in some of our distributions, as where the numbers are not large both the mean and the standard deviation are subject to substantial variation, and the terms neglected are of weight. Since the conclusions deduced in the text from Table VI depend more on the general agreement in size of the values of ρ than on their probable errors, we have not attempted to go beyond this approximation to the latter.

*[2] In our case there is a positive correlation, hence the approximation is too large.

APPENDIX IV.

Approximation to the Probable Error for use with Table VII.

Let N be the number of persons observed.
,, p be the proportion of people in the zero group.
,, m' be the theoretical mean deduced from p by Table VII.
,, m be the observed mean of the accident distribution.
,, σ be the standard deviation of the accident distribution in the whole population from which the observations are a sample.

We want to find Σ, the standard deviation of the difference $m-m'$, between the observed and theoretical means.

$$\Sigma^2 = \sigma_m^{\,2} - 2r_{mm'}\,\sigma_m\,\sigma_{m'} + \sigma_{m'}^{\,2} \text{ (i)}$$

Now $\sigma_m^{\,2} = \dfrac{\sigma^2}{N}$ (ii)

To get $\sigma_{m'}^{\,2}$

$$p = e^{-m'}$$

$$\therefore m' = -\log p$$

$$\therefore \delta_{m'} = -\frac{\delta p}{p} \text{ to a first approximation}$$

$$\therefore \delta^2_{m'} = \frac{\delta p^2}{p^2}$$

Taking mean values and writing $\dfrac{pq}{N}$ for $\sigma_p^{\,2}$ where $q = 1 - p$

We get $\sigma_{m'}^{\,2} = \dfrac{\sigma_p^{\,2}}{p^2} = \dfrac{q}{Np}$ (iii).

To get $r_{mm'}\,\sigma_m\,\sigma_{m'}$

$$\delta m\,\delta m' = -\frac{\delta m\,\delta p}{p}$$

$$\therefore r_{mm'}\,\sigma_m\,\sigma_{m'} = -\frac{r_{mp}\,\sigma_m\,\sigma_p}{p}$$

But $r_{mp}\,\sigma_m\,\sigma_p = -\dfrac{pm}{N}$ by eqn: (ix), p. 278 of Biometrika II

$$\therefore r_{mm'}\,\sigma_m\,\sigma_{m'} = \frac{m}{N} \text{ (iv)}$$

Substituting in (i) we get—

$$\Sigma^2 = \frac{\sigma^2}{N} - \frac{2m}{N} + \frac{q}{Np}$$

$$\therefore \Sigma = \frac{1}{\sqrt{N}}\left(\sigma^2 - 2m + \frac{q}{p}\right)^{\frac{1}{2}}$$

$\therefore$ Probable error of the difference between the observed and theoretical means is $\dfrac{\cdot 67449}{\sqrt{N}}\left(\sigma^2 - 2m + \dfrac{q}{p}\right)^{\frac{1}{2}}$.

We can approximate to σ^2 by taking the observed standard deviation, or to get a rough result quickly, we can assume that the pure chance theory, i.e., the theory we are testing, holds, so that $\sigma^2 = m$ and the probable error reduces to $\dfrac{\cdot 67449}{\sqrt{N}}\left(\dfrac{q}{p} - m\right)^{\frac{1}{2}}$, or if percentage values are preferred this can be written $\dfrac{\cdot 67449}{\sqrt{N}}\left(\dfrac{Q}{P} - m\right)^{\frac{1}{2}}$, where $Q = 100 - P$.

It may happen that $\dfrac{q}{p} - m$ is negative, if this is so it shows that the pure chance theory does not hold.

Appendix V.

Card used for the Collection of the Data.

Front of Card.

Factory Code No.

Name

Check No.

Sex

Civil State

Date of Birth

Date of Entry

Date of Leaving

Department

Occupation

Any other information

Accidents of all kinds, trivial and serious. **C** = Compensated. **U** = Uncompensated

No.	Date	Time	Nature of Injury	Cause	Occupation at the time of injury	C or U	No. of days lost
1							
2							
3							
4							
5							
6							
7							
8							
9							
10							
11							
12							
13							
14							

Minor Sickness treated in Ambulance Room.

No.	Date	Nature of Sickness	No.	Date	Nature of Sickness
1			7		
2			8		
3			9		
4			10		
5			11		
6			12		

Appendix V.—*contd.*

Back of Card.

	TIMEKEEPING RECORD					OUTPUT RECORD	
	LATENESS		ABSENCE				
	Times	Hours	Sickness days	Other Reasons days	Total days	Output, Wages or Bonus	
1st 4 wks.							
2nd ,,							
3rd ,,							
4th ,,							
5th ,,							
6th ,,							
7th ,,							
8th ,,							
9th ,,							
10th ,,							
11th ,,							
12th ,,							
13th ,,							
Total							

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